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EDINBURGH
MEDICAL AND SURGICAL
JOURNAL:

EXHIBITING

A CONCISE VIEW

OF THE

LATEST AND MOST IMPORTANT DISCOVERIES

IN

MEDICINE, SURGERY, AND
PHARMACY.

VOLUME SIXTY-NINTH.
1848.

EDINBURGH:
PRINTED FOR ADAM AND CHARLES BLACK:
LONGMAN, BROWN, GREEN, & LONGMANS, LONDON:
JOHN CUMMING, AND HODGES & SMITH, DUBLIN.

1848.

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THE

EDINBURGH

MEDICAL AND SURGICAL JOURNAL.

No. CLXXIV.

CONTENTS.

PART I.—ORIGINAL COMMUNICATIONS.

ART. I. An Account of the Anomalies of the Muscular System, met with in the Dissecting-room of the University during the years 1846–1847; with general Remarks. By C. H. HALLETT, Surgeon, Page 1
II. Observations on the Climate of Largs, with Notices of the other principal Watering-places on the Clyde, and their general Characteristics. By WILLIAM DAVIDSON, M. D. . . . 32
III. Notes of the Cases of several Men who were Wounded in the different Engagements on the Sutlege, and its vicinity. With Remarks. By GEORGE WILLIAMSON, M. D.. 51
IV. On the Period of Puberty in the Negro. By JOHN ROBERTON, Surgeon, ... 69
V. Case of Dislocated Head of Radius successfully reduced two years and one month after the occurrence of the Dislocation. By JAMES STARK, M. D. 77
VI. Report of some cases of Molluscum Contagiosum, with observations on its General History and Pathology. By RICHARD PAYNE COTTON, M. D. 82
VII. Cases illustrating the Nature and Character of Inflammation and Suppuration of the Heart. By DAVID CRAIGIE, M. D. . 86

PART II.—CRITICAL ANALYSIS.

ART. I. Report on the Climate and Principal Diseases of the African Station; compiled from Documents in the office of the Director-General of the Medical Department, and from other sources, in compliance with the Directions of the Right Honourable the Lords Commissioners of the Admiralty. Under the immediate Direction of Sir William Burnett, M. D. By ALEXANDER BRYSON, M. D. 107

Page
ART. II. Urinary Deposits, their Diagnosis, Pathology, and Therapeutical Indications. By GOLDING BIRD, A. M. . . . 138
III. On the Causes and Treatment of Abortion and Sterility. Being the Result of an Extended Practical Inquiry into the Physiological and Morbid Conditions of the Uterus, with reference especially to Leucorrhœal Affections and the Diseases of Menstruation. By JAMES WHITEHEAD, F. R. C. S. 169
IV. 1. Observations on the Anatomy of the Lungs. By THOMAS ADDISON, M. D.
2. On the Ultimate Distribution of the Air-Passages and Air-Cells of the Lungs. By WILLIAM ADDISON, Esq.
3. Extrait d'un Memoire sur la Structure intime des Poumons dans l'homme et les Mammiferes, (lu a l'Academie des Sciences, 11 Juillet 1842). By J. M. BOURGERY.
4. On the Minute Structure of the Lungs, and on the Formation of Pulmonary Tubercle. By GEORGE RAINEY, Esq. . 192
V. Births and Deaths registered in London during the weeks ending Saturday, 13th November, 20th November, 27th November, 4th December, and 11th December 1847, . . . 214
VI. Clinical Facts and Reflections; also, Remarks on the Impunity of Murder in some cases of Presumed Insanity. By THOMAS MAYO, M. D. 217
VII. Speech of the Count de Montalembert in the House of Peers of France, the 5th June 1847, on the subject of Medical Reform; with Notes of the Translator, ... 233
VIII. 1. Elements of Chemistry, including the Actual State and Prevalent Doctrines of that Science. By the late EDWARD TURNER, M.D.
2. A Manual of Elementary Chemistry, Theoretical and Practical. By GEORGE FOWNES, F. R. S. 238

PART III.—MEDICAL INTELLIGENCE.

On the Pacinian Bodies (Corpuscula of Pacini). Muller's Archiv, 1846, 241
Chloroform as a means of producing Insensibility to Pain during severe operations, ... 243
Cholera at Moscow, ... 249
Effects of the Disinfecting Fluids of Sir William Burnet and Mr Ledoyen, 250
Instance of Rupture of the Urinary Bladder terminating in recovery of the patient, ... 253
Temperature, &c. of Penetanguishene, Canada, in 1846 and 1847, . 255
Metropolitan Sanitary Commission, ... ib.
Report on the Mortality of Edinburgh and Leith, for the months of September, October, and November 1847, ... 263
Death of Mr Liston, ... 267

NOTICES TO CORRESPONDENTS.

Communications have been received from Dr K. **Bennet**, Mr **Reynolds**, and Dr A. **Bertram**.

The following Publications have been received.

Theorie des Neuroviscerites, ou Fievres Primitives, Prouvée par l'Examen approfondi de la Lesion des Proprietés Vitales dans les Parties Organiques, qui en sont la siege et par l'Experimentation Physiologique. Par Ant. Hugon, Medecin au Crest (Puy-de-Dome). Paris, 1847. 8vo. Pp. 112.

On Tumours of the Uterus and its Appendages, (Jacksonian Prize Dissertation). By Thomas Safford Lee, M. R. C. S. E. and London. 1847. 8vo. Pp. 274.

An Essay on the Use and Abuse of Restraint in the Management of the Insane, containing some Remarks on the Origin and Nature of their Disease; with Copious Notes. By Hamilton Labatt, A. B., T. C. D., Fellow of the Royal College of Physicians. Ireland, Demonstrator of Anatomy at that College, &c. Dublin and London, 1847. 8vo. Pp. 76.

A Letter to Benjamin Rotch, Esq., Chairman of the Committee of Visitors, on the Plan and Government of the Additional Lunatic Asylum for the County of Middlesex, about to be erected at Colney Hatch. By John Conolly, M. D., Fellow of the Royal College of Physicians of London, and Physician to the Middlesex Lunatic Asylum at Hanwell. London, 1847. 8vo. Pp. 27.

Researches on the Pathology and Treatment of Cholera. By E. A. Parkes, M. D., London, Assistant Physician to University College Hospital. London, 1847. 8vo. Pp. 250.

An Essay on the Diseases of the Jaws, and their Treatment. By Leonard Koecker, Surgeon-Dentist, D. M. and Chir. &c. &c. New Edition, with Copious Notes and an Appendix, containing Tables of upwards of 300 Cases. By J. B. Mitchell, M. D., Surgeon-Dentist. London, 1847. 8vo. Pp. 94.

On Indigestion; its Pathology and Treatment by the local application of Uniform and Continuous Heat and Moisture; with an Account of an Improved Mode of applying Heat or Cold in Irritative and Inflammatory Diseases. By James Arnott, M. D., Physician to the Brighton Dispensary. London, 1847. 8vo. Pp. 107.

A Treatise on Diet and Regimen. By William Henry Robertson, M. D., Physician to the Buxton-Bath Charity. Fourth Edition. Re-written and much enlarged. London, 1847. Part IV. December. Post 8vo. Pp. 120.

On Poisons, in relation to Medical Jurisprudence and Medicine. By Alfred S. Taylor, F. R. S., Lecturer on Medical Jurisprudence and Chemistry in Guy's Hospital. London, 1848. 12mo. Pp. 855.

The Pocket Formulary and Synopsis of the British and Foreign Pharmacopœias; comprising Standard and approved Formulæ for the Preparations and Compounds employed in Medical Practice. By Henry Beasley. Fourth Edition, corrected, improved, and enlarged. London, 1848. 12mo. Pp. 455.

Du Cholera; Moyens Preservatifs et Curatifs; ou Philosophie des Grandes Epidemies. Par A. M. Bureaud-Rioffrey, D. M. de la Faculté de Paris, &c. &c. Paris, 1847. 8vo. Pp. 116.

Lectures on the Physical Phenomena of Living Beings. By Carlo Matteucci, Professor in the University of Pisa. Translated under the superintendence of Jonathan Pereira, M. D., F. R. S. &c. London, 1847. 12mo. Pp. 435.

On Ringworm: its Causes, Pathology, and Treatment. By Erasmus Wilson, F. R. S., Consulting Surgeon to the St Pancreas Infirmary. London, 1847. Post 8vo. Pp. 102.

Deafness Practically illustrated. Being an Exposition of Original Views as to the Causes and Treatment of Diseases of the Ear. By James Yearsley, M. R. C. S. E. &c. London, 1847. Post 8vo. Pp. 181.

Report of the Directors of the Montrose Lunatic Asylum, Infirmary, and Dispensary, for the year ending 1st June 1847. Montrose, 1847. 8vo. Pp. 26.

A Practical Treatise on the Causes, Symptoms, and Treatment of Spermatorrhœa. By M. Lallemand, formerly Professor of Clinical Surgery at the University of Montpellier, &c. Translated by Henry J. M'Dougall, M. R. C. Surgeons, Ireland, &c. &c. London, 1847. 8vo. Pp. 333.

Report of the Proceedings of the Pathological Society of London. First Session, 1846—47. London. 8vo, pp. 155.

Physiology of the Nervous System. By Robert B. Todd, M. D., F. R. S., F. R. C. P., &c. &c. From the Cyclopædia of Anatomy and Physiology. London, 1847. Large 8vo, pp. 72.

On the Contractility or Irritability of the Muscles of Paralysed Limbs, and their Excitability by the Galvanic current, in comparison with the corresponding Muscles in Healthy Limbs. By Robert Bentley Todd, M. D., F. R. S., &c. &c. 8vo. London, 1847. Pp. 21. (Medico-Chirurgical Trans. Vol. xxx.)

Observations on Pleuritis and Empyema in Children; with cases. By Francis Battersby, M. B. T. C. D., F. R. C. P. Ireland, &c. (Dublin Quarterly Journal., No. viii.) Dublin, 1847. 8vo, pp. 27.

An Introductory Lecture, read to the Medical Classes in King's College, London, October 1, 1847. By George Budd, M. D., F. R. S., and Professor of Medicine in King's College, London, &c. London 1847. 8vo.

On the Nucleus of the Animal and Vegetable Cell. By Martin Barry, M. D., F. R. S. Edin. 1847. 8vo, pp. 29. (Edin. New Philosophical Journal, October 1847.)

Ventilation illustrated. A Tract for the Schools of Rich and Poor. London, 1848. 12mo. pp. 36.

Alveolar Hemorrhage Compress; constructed by Dr R. Reid, Dentist, Edinburgh. With an engraving. 8vo, pp. 3.

The Medico-Chirurgical Review and Journal of Practical Medicine. New Series, No. 12. 94 of old Series. October, 1847. London.

The British and Foreign Quarterly Review, or Quarterly Journal of Practical Medicine and Surgery. Edited by John Forbes, M. D., &c. No. 48. October, 1847. London. 8vo.

The London Medical Gazette, or Journal of Practical Medicine. No. 126 and 1033, 25th September 1847, to No. 132 and 1040, 5th November. Other Numbers not received, except 137, 10th December 1847.

The Dublin Quarterly Journal of Medical Science. No. viii. November, 1847. Dublin. 8vo. No. vi. May, and No. vii. August not received.

Weekly Tables of Mortality for the Metropolis. No. 39, September 25, 1887—No. 50, 11th December 1847. Annual Series, viii.

The American Journal of Medical Sciences. Edited by Isaac Hays, M. D., Surgeon to Wales' Hospital, &c. No. xxvii. New Series. July, 1847. Philadelphia.

THE

EDINBURGH

MEDICAL AND SURGICAL JOURNAL.

1. JANUARY 1848.

PART I.

ORIGINAL COMMUNICATIONS.

ART. I.—*An Account of the Anomalies of the Muscular System, met with in the Dissecting-room of the University during the years* 1846–1847; *with general Remarks.* By C. H. HALLETT, Surgeon, Demonstrator of Anatomy in the University of Edinburgh.

IN giving the following account of the various irregularities of the muscular system that have been noticed in the dissecting-room of the University during this and the preceding year, it is incumbent on me to state that I have been actuated not so much by the desire of describing some anomalies, which would appear to have hitherto escaped the notice of anatomists, as by the wish to increase the number of facts already recorded concerning those which are better known, and at the same time to draw the attention of those who have opportunities of making similar observations to the interest attached to the subject, not only in a teratological, but also in a physiological and practical point of view.

Musc. Retrahens Aurem.—In three subjects[*] a remarkable development of this muscle has been observed. The lower fasciculi, generally attached to the base of the mastoid process, had united into a round-bellied muscle, which was attached to the occipital protuberance, and to the inner part of the superior curved line of the occipital bone, on the one hand, and to the *concha auris*, distinct from the upper fasciculus, on the other. It lay in a sheath formed of condensed cellular membrane between the tendons of the occipito-frontalis and the sterno-cleido-mastoid muscles, and was supplied by the same branch of the posterior auricular nerve that is furnished to the former muscle.

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[*] As frequent mention will be made of the number of times that certain anomalies were observed, and of the relative frequency of some others, it may be as well to mention that the number of subjects actually dissected in the rooms, from May 1846 to July 1847,—the time over which my observations extend—was one hundred and five.

In many other subjects a tendency to the formation of this abnormal muscle has been indicated, by some of the lower fibres of the *retrahens aurem* passing on to the occipital bone,—a slight form of the anomaly noticed by most anatomists.

Musc. Depressor Anguli oris.—This muscle was frequently anomalous. In several instances the more superficial fibres of this muscle, on both sides of the face, were not attached to the lower jaw, but arched inwards, downwards, and forwards, beneath the chin, and became continuous with each other in the mesial line immediately below and behind the *symphysis menti*. Where this anomaly existed, the two muscles in unison seemed to form a sling which supported the chin.

The abnormal fibres were always interlaced with the innermost fibres of the *platysma myoides*, and in such a manner that, without dissection, they might be mistaken for fibres uniting the *platysma myoides* of opposite sides. Fleischmann,* Gautzer,† Meckel,‡ and others, have noticed and described such fibres; but I have no doubt, from my own dissections, that they constituted the anomaly just described, especially as they make no mention of it in their works. Cast, No. 1.§

Musc. Digastricus.—This muscle presented very frequently—about once in every fifteen subjects—a third belly, and that always on both sides of the neck. The belly commenced by a short flattened tendon at the anterior part of the reflection of the tendon, between the two normal bellies. The muscular fibres fixed to this tendon spread out into a pretty thick fan-shaped muscle, passed obliquely forwards, inwards, and upwards, and terminated by uniting, in the mesial line of the neck, with the corresponding fibres of the opposite side. The superior fibres were always the longest, and in one instance ascended to within a line or two of the *symphysis menti.* I have succeeded, on several occasions, in tracing filaments from the mylo-hyoid nerve into this abnormal belly.

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* Erlangen Abbandlung, vol. i. p. 28.

† Dissertatio anatomica musculorum varietates sistens, page 6.

‡ Handbuch der Menschlichen Anatomie, translated by Jourdan and Breschet. Myologie, artic. iii.

§ I have thought proper to notice those varieties of which casts have been taken. The casts are placed in the Museum of the University, where they may be inspected by those who have an interest in the subject. The numbers refer to the numbers on the casts.

This anomaly has been noticed by many anatomists; is said by Meckel to represent the normal condition of the muscle in many of the mammalia; and there can be no doubt that it—the muscle—occupies the place of the supra-hyoid aponeurosis. Cast, No. 2.

Musc. Stylo-Hyoideus.—The insertion of this muscle was found on two occasions to deviate from the normal state. The muscle, near the place where it usually divides to allow the tendon of the digastricus to pass through it, separated into two portions, the lower of which was inserted into the body of the hyoid bone, while the upper was firmly and inseparably united to the intermediate tendon of the digastricus.

Musc. Omo-Hyoideus.—The varieties in the attachment, the position, and the number of bellies of the omo-hyoid muscle, were frequently met with in various grades of complexity, the simplest as well as the most complicated being of the greatest importance to the surgeon, in consequence of the material alterations induced by them in the boundaries and relations of the principal surgical regions of the neck.

The first variety, one affecting the position of the posterior belly, was caused not by any fault in the muscle itself, but in the layer of the deep cervical fascia which ties the intermediate tendon down and preserves the angular direction of the muscle. The variety in question was very frequently met with, being found about once in every five subjects, and consisted merely in the posterior belly of the muscle being brought nearly, if not in contact, with the clavicle, by the layer of the cervical fascia just alluded to. It of course entirely did away with the posterior inferior triangle of the neck, and it appeared to form the transition between the normal state of the muscle and the next variety.

The second variety, the first of those causing an alteration in the attachment of the posterior belly, although more rare than the preceding, was far from being uncommon, inasmuch as it was met with about once in every fifteen subjects. The posterior belly in those cases had a direct attachment to the posterior face of the middle part of the clavicle, by means of muscular fibres distinct from those coming from the scapula, and in some cases forming a distinct belly, the fibres of which, however, mingled with the proper fibres of the posterior belly, before the intermediate tendon was produced. In the latter case an additional belly appeared to be superadded, but in all, the posterior inferior triangle of the neck either did not exist, or its appearance was very materially altered. Albinus[*] has described this variety. Kelch[†] has also noticed it, with this modification, that the portion attached to the clavicle was intermixed with tendinous fibres.

The preceding variety would appear to be a transition from the first to another still more grave, in which the posterior belly of the muscle is attached to the clavicle, and not to the scapula. In such anomalies it was observed that no intermediate tendon existed, but that the muscle consisted of a single broad and flattened belly, passing obliquely from the middle of the clavicle to the hyoid bone, or uniting with the fibres of the sterno-hyoid muscle about midway between the hyoid bone and the sternum. In either case the subclavian artery was entirely concealed. This variety was but rarely seen. It has been noticed by Albinus[‡] and others. Cast, No. 3.

All the varieties just described affected only the boundaries of the posterior triangle of the neck. There were others, however, which altered the boundaries of and the relations of the parts in the anterior triangle. These consisted in the total deficiency of the anterior belly of the muscle, or in the absence of the muscle, the former having been seen twice, the latter once, and then only on one side of the neck. In both varieties the sterno-hyoid muscle was always much broader and thicker than usual, and appeared to supply the place of the omo-hyoid or its anterior belly, by sometimes being attached to the clavicle behind the sterno-mastoid. The first of these varieties consisted in the posterior belly terminating in the cervical fascia, near the place where the intermediate tendon usually exists; and the muscle in such cases has been called, I believe by Krause, the coraco-cervical.

Besides these, others were seen, in which the omo-hyoid, while apparently normal, sent off or was connected with accidental slips of muscle. One of these, which has been seen and described by Soemmering,[§] was a considerable slip of muscle connected to the omo-hyoid at its origin from the scapula by a short thick tendon, and which, after traversing the subclavian space in contact with and anterior to the brachial plexus and the subclavian artery, was attached by its other extremity, also by means of a short tendon, to the cartilage of the first rib. Another consisted in a very slight modification of the median attachment of the preceding, which, instead of going to the first rib, was fixed to the upper part of the sterno-clavicular articulation. In its course it ran through the very centre of the posterior inferior triangle. In one subject, the posterior belly sent off a slip of muscle which was intimately blended with the fibres of the sterno-mastoid; and in another, a fasciculus which was partly attached to the transverse process of the sixth cervical vertebra, and partly lost in the substance of the *scalenus anticus.*

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[*] Historia Musculorum Hominis, caput xxxviii. De coraco-hyoideo.

[†] Beitraege zur pathologischen Anatomie, p. 81. See also Winslow's Exposition anatomique de la structure du corps humain.

[‡] Loc. cit.

[§] De Corporis Humani Fabrica, Vol. iii. p. 113.

Of the numerous varieties in the extent of the attachment of the different bellies, in the size and development of the intermediary tendon, and in the dimensions of muscle, it is unnecessary to say anything, inasmuch as they are of no importance.

Musc. Sterno-Hyoideus.—In describing the varieties of the omo-hyoid, it has been stated that conjointly with deficiency wholly or in part of that muscle there was an anomaly in the attachment of the sterno-hyoid. This anomaly in the attachment of the sterno-thyroid was observed in two instances to be carried to a much greater extent without any abnormal condition of the omo-hyoid, showing, in fact, that although sometimes associated, the anomalies may be independent of each other. In the two instances just mentioned, the muscle on both sides of the neck, instead of being attached to the sternum and first rib, was fixed to near the middle of the clavicle. The direction of the muscle was consequently altered considerably; it was more oblique than usual; and in consequence of this, the common carotid artery and the internal jugular vein were completely concealed by muscle until they had emerged from behind the omo-hyoid. Kelch* has also noticed this remarkable and important anomaly. Cast, No. 4.

This muscle was observed on several occasions to be intersected by one or more tendons, either partial or complete.

Musc. Sterno-Thyroideus.—This muscle, like the preceding, was occasionally intersected by partial or complete tendons. The principal anomaly, however, was found in a subject in which the greater part of the muscles had been transformed into fat. The muscle in it was double throughout its whole extent—the abnormal one being smaller and superimposed on the other—a complete cellular interval separating the two. A tendency to this was observed on two other occasions; the muscle being double below, but single above. The muscle also presented a fissure in its lower part, exhibiting a tendency to the formation of a double muscle, by splitting in the direction of its length.

The former of these anomalies has been noticed by Gunz.† The latter is described with some others by Soemmering.‡

Musc. Glandulæ Thyroideæ.—This muscle, which is not always described in works on anatomy, and appears to have escaped the notice of Albinus, must be considered an abnormal development, inasmuch as it is but seldom met with, and even then is extremely variable. The most usual form in which it presented itself was as a fasciculus of muscular fibres placed in the mesial line of the neck, and stretching between the hyoid bone and the isthmus of the thyroid gland. It appeared as an unsymmetrical muscle; but that it is to be considered as a double muscle in which the line of separation cannot always be distinguished, appears to be proven by some of its varieties. Thus, in some subjects it was completely double, each half being in close apposition and mixed up with the fibres of the hyo-thyroid muscle—a circumstance which shows that it is an offset of that muscle. In others, the two portions were more closely approximated, either above or below; or again, it was single above and bifid below; or was single below and bifid above. It was sometimes present only on one side; or was fully developed on the one side, but rudimentary on the other.

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* Loc. cit. page 32.

† Mem. de l'Academie des Sciences, vol. i. page 286.

‡ Op. cit. page 116.

Musc. Mylo-Hyoideus.—Most anatomists have remarked the occasional intimate connection of this muscle to the intermediate tendon of the digastricus. It was observed by myself to happen about once in every five subjects. An uncommon anomaly of this muscle consisted in its almost total absence, when it was replaced by an increased breadth and thickness of the anterior belly of the digastricus.

Musc. Genio-Hyoideus.—The genio-hyoids had in some subjects become so intimately blended, and the usual cellular interval between them so completely removed, that they appeared as one muscle. This was by no means an unfrequent anomaly, but was only seen in very muscular subjects. The very opposite state of matters was presented by two subjects, also remarkable for the development of the muscular system. In these, the muscle on both sides was double, so that there were four genio-hyoids in all, and neither of these were much, if anything, smaller than the muscle in its normal state. Meyer* has particularly noticed this anomaly.

Musc. Sterno-cleido-Mastoid.—The anomalies of this muscle presented no great variety, but they were far from being uncommon. The most frequent anomaly was an increase in the attachment of the sterno-cleido-mastoid to the clavicle, the fibres extending outwards to near the middle of the clavicle, and in one instance, where it was very thin and broad, to near the commencement of the outer third of that bone. The proportion of this anomaly to the normal state was as one to eight, and in one-half of these, the clavicular portion of the trapezius was also extended so as to meet the sterno-cleido-mastoid, and with it almost entirely concealed the posterior inferior triangle of the neck. In three of these instances, the presence of a third head appeared to have been the cause of this increased attachment. This third head was small, and placed between the normal sternal and clavicular heads, and blended with these at variable distances from the mastoid attachment of the muscle.

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* Beschreibung des Menschlichen Korpers, vol. iii. p. 547.

In some few instances, the sternal and clavicular heads were intimately united at their commencement. In others, they were separated much farther up than ordinary—sometimes to within an inch of the mastoid process.

Rarely, the sternal head, instead of being attached to the sternum, was continuous with the uppermost tendon of a remarkable anomalous muscle, the *musculus sternalis* or *rectus brutarum*, of which particular mention will be made hereafter.

Musc. Trapezius.—This muscle was frequently found deficient either above or below, and in one instance was observed almost altogether absent. In almost all these cases the deficiency was only on one side, and that generally the left. When deficient below, the muscle was attached to eight, nine, or even ten dorsal vertebræ, but sometimes only to three or four. When the deficiency was above, the muscle seldom ascended higher than opposite the spinous process of the second cervical vertebra.

In the subject where it was almost entirely deficient, the trapezius of the left side descended as far as the third dorsal vertebra, terminating on a line drawn from it to the spine of the scapula. From that point a thin but firm fascia was prolonged downwards, which appeared to represent the lower portion of the muscle. Superiorly, the muscular fibres were deficient above the spinous process of the third cervical vertebra.* The trapezius of the right side was also deficient to the same extent above, but descended as far as the spinous process of the tenth dorsal vertebra below. The muscle on both sides was remarkably thin, more especially the left, the fibres of which were scarcely discernible.† Cast, No. 5.

Where the superior portion of the muscle was deficient, the clavicular attachment was always less than usual. Some subjects, however, in which the muscle was remarkably strong, presented a greater clavicular attachment than ordinary, so much so, indeed, in some, as to occupy the middle as well as the outer third of the clavicle, and to come in contact with the outer head of the sterno-cleido-mastoideus.

On two occasions, a large fasciculus was detached from the anterior border of the muscle near its lower attachment, which passed over the posterior inferior triangle, and was attached to the sternal end of the clavicle immediately behind the sterno-cleido-mastoideus.* It is scarcely necessary to mention that both this and the preceding anomaly cause a marked alteration in the surgical region of which the trapezius forms a boundary.

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* Described also by M'Whinnie.—Varieties in the muscular system of the human body.—London Medical Gazette, No. 948. January 30, 1846.

† The trapezius in this condition resembles the trapezius of the Sapajous, in which it scarcely ever descends beyond the second dorsal vertebra.

Musc. Latissimus Dorsi.—The attachment of this muscle to the inferior angle of the scapula was seldom seen, not oftener than once in every four subjects, so that when present it ought to be considered as an abnormal attachment. Its origin from the lower ribs was also variable as regards number. The number of fasciculi attached to the ribs was usually three, in some cases only two, but oftener four. In some of those subjects where it was attached only to the two last ribs, it was attached to the four lower dorsal vertebræ; this variety, however, sometimes existed where the attachment to the ribs was normal. The muscle was observed on three occasions to be fixed to the spinous processes of the eight inferior dorsal vertebræ, but this occurred on one side only.

Besides receiving abnormal bundles of muscle from the *pectoralis major*, of which mention will be made hereafter, it occasionally sent off fasciculi to that muscle. Such fasciculi generally passed off from it near its humeral attachment, and arching forwards over the axillary vessels and nerves, terminated in the tendon of the *pectoralis major*. Meckel† describes this as the normal state of the muscle in the mole and in birds; and Dr Ramsay‡ presumed that it might compress the arteries and nerves during violent exertion, and might account for the liability to swelling of the axillary glands (?) on such occasions in some people.

M. Rhomboideus Major.—The origin of this muscle was observed about once in every thirty subjects to be less than ordinary—the attachment to the spinous processes of the third and fourth dorsal vertebræ being deficient. It was seen in one subject to overlap the origin of the *rhomboideus minor* on both sides of the back, and to be fixed to the spinous process of the seventh cervical vertebra and a small portion of the *ligamentum nuchæ*.

M. Sternalis Brutorum.—The *sternalis brutorum*, *rectus sternalis*, or thoracicus, as it has been variously denominated by different anatomists, has been seen from its lowest to its highest grade of development. Of all the abnormal muscles in the human body, this is probably the most interesting, on account of its occasional large size, its position and attachments, its general anatomical relations, and its use, and therefore I shall describe at some length those examples of it which have fallen under my notice.

In many subjects a tendency to the formation of this muscle was exhibited by the existence of a firm band of fascia stretching obliquely from the cartilage of the third rib to the sheath of the rectus.[*] About one in every fifteen subjects presented this band of fascia developed into muscle, which, usually stretching between the points just stated, or being placed more external, and then passing between the second and the fourth costal cartilages superficial to the *pectoralis major*, between it and the fascia, was the lowest grade of development of the muscle seen by myself, and I believe by others.[†]

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* This anomaly is figured in Quain's plates of the arteries, plate 25.

† Op. cit., "grand dorsal."

‡ Account of Unusual Conformation of some Muscles and Vessels.—Edin. Med. and Surg. Journal, vol. viii. p. 281. The relative frequency of this variety is stated both by Ramsay and Meckel to be one in thirty, which is pretty correct.

The next stage observed by myself was as follows:—The muscle commenced by a strong round tendon attached to the upper and right side of the manubrium, and by muscular fibres which were blended with those of the *pectoralis major* of the same side. The strong tendon soon terminated in a thick muscular belly, which united with the other muscular fibres coming from over the *pectoralis major*, and the whole then passed obliquely downwards and outwards, and to the left side. Having descended as low as the four costal cartilages, the muscle sent off three tendons, which severally attached themselves to the fourth and fifth costal cartilages, and to the extremity of the sixth rib between the attachments of the *pectoralis major* and the *rectus abdominis*, with which some of the muscular fibres were directly continuous. The length of this muscle was above four inches, and its breadth about an inch and a half.[‡]

Another subject presented the largest and most striking example of this remarkable muscle that has probably ever been observed. The muscle extended from the upper border of the sternum to the ensiform cartilage and sixth rib, and caused a considerable projection on the right side of the chest, visible before the skin was removed, and rendered more apparent by a depression of the lower part of the sternum and a twisting forwards of the ensiform cartilage. It was about six inches long, two inches broad, and half an inch thick.

The muscle arose by three distinct heads. The first or superior head was the largest; it commenced by a rounded tendon continuous with the tendons of the sternal heads of both sterno-cleido-mastoid muscles. The tendon of the left sterno-cleido-mastoid was directly continuous with it, in fact no line of demarcation could be drawn between them; it had but a slight attachment to the sternum, and that only by means of a short thin tendinous band which passed outwards, and seemed to belong as much to the *sternalis brutorum* as to it. The attachment of the right sterno-cleido-mastoid to the sternum was more complete, although its appearance was similar to the left; its continuity with the tendon of the *sternalis brutorum* was not so easily demonstrated as that of the left, but was shown by pulling either tendon. The second or middle head arose tendinous from the upper part of the manubrium to the left of the mesial line, and was connected by slips of tendon to some fibres of the *pectoralis major* of the left side, and also to the superior head. The third or inferior head, also tendinous, arose from the sternum opposite to the third costal cartilage, the fibres of the tendon interlacing with the fibres of the anterior chondro-sternal ligaments, which were unusually well developed.

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[*] Other anatomists have seen it represented by a thin band of tendon passing between the sterno-cleido-mastoid and abdominal aponeurosis. See Meckel, op. cit. and others.

[†] An engraving of the muscle at this stage will be found in Haller, Icones Anatom. Fasc. vi. Tab. 1, N.

[‡] Wilde in Commen ariis Petropolitanis, vol. xii. Tab. 8, fig. 5, delineates the muscle at this stage.

The muscular bundles attached to these tendons formed a large mass of muscle which passed obliquely downwards and outwards for some distance, and then suddenly turned inwards towards the mesial line. The muscle was inserted by numerous fasciculi into the lower costal cartilages, into the abdominal aponeurosis, the ensiform cartilage and the costo-ziphoid ligaments, all the fasciculi passing off from its posterior aspect. The muscular fibres connected with the middle head attached themselves principally to the third costal cartilage, but were also prolonged over it to the fourth costal cartilage. The fibres belonging to the superior head fixed themselves to the fourth, fifth, and sixth costal cartilages, each fasciculus sending off a tendinous prolongation to the cartilage immediately below. The remainder of the muscle was attached to the sheath of the rectus, which in this instance was strengthened by an arched band of tendinous fibres crossing it transversely to the fifth and sixth anterior chondro-sternal ligaments, to the costo-ziphoid ligament and to the ziphoid cartilage, the fasciculi attached to these parts severally passing over and external to each other, and being arranged like a series of steps.

The third, fourth, and fifth intercostal nerves supplied the muscle with nervous influence. They entered it by its posterior surface. The intercostal and mammary arteries supplied it with blood.* Cast, No. 6.

The smaller muscles have been seen several times double on one side, and sometimes on both sides, but I have never met with more than one of the larger muscles in the same subject.

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* There are many other modifications of this muscle which have not come under my notice, and which I have, therefore, abstained from mentioning. Those who are desirous of knowing more concerning it will find notices of it by Sandifort—de musculis nonnullis qui rarius occurrunt, in the Exerc. Acad. vol. i. cap. 6, page 82–88; by Meckel—De monstrosa duplicitate, 1815, page 38–40; by Kelch—op. cit. page 33; and besides those already mentioned, they will find engravings in Novi. Comment. Petroph. vol. ii. tab. 2, fig. 11; and tab. 12; by Abr. Kaau-Boerhaave; and by Rosenmuller, in Neuflamm and Rosenmuller's Beitrage fur die Zergliederungskunst.

The *sternalis brutorum* is presumed to be a more or less perfect repetition of the *rectus abdominis* and of the sterno-cleido-mastoideus, with both of which muscles it is, as just shown, sometimes continuous. The analogy is most striking in some cases where the muscle prevents tendinous intersections, as observed by Meckel,* and seen once by myself. It is also supposed to establish a relation between man and animals where the *rectus abdominis* is sometimes prolonged upwards.

When fully developed, it can act as an elevator of the ribs, and, as I shall show when I come to speak of the use of supernumerary muscles in general, may be of great service under certain circumstances, by acting as a muscle of inspiration.

Musc. Pectoralis Major.—Besides having an abnormal attachment to the *latissimus dorsi* by means of fasciculi detached from the latter muscle, the *pectoralis major* occasionally sent off slips of muscle to the *latissimus dorsi.* These were for the most part small, and undeserving of any particular notice; some, however, were large and remarkable.

In one subject the greater part of the base of the axilla was floored with muscular fibres, part of which belonged apparently to the *pectoralis major*, and part to the *latissimus dorsi.* A large and broad fasciculus of muscular fibre arose from the anterior extremity of the sixth and seventh ribs, and from the intercostal space between, and ran outwards across the axilla, immediately behind the *pectoralis major*, of which it appeared to be a part, and from which it was separated by a cellular interval. Near the middle of the axilla it was joined by a larger and broader mass of muscle detached from the *latissimus dorsi*, which being about two inches broad and three inches in length, came partly from the tendinous, partly from the muscular portion of the *latissimus dorsi.* The muscular fibres, however, were not continuous, for short tendinous fibres intervened between them. The two having united, an aponeurotic tendon was formed, which passed under the *pectoralis major*, and attached itself to the tendon of that muscle and to the fascia of the arm. It may be as well to mention that the *latissimus dorsi* in this subject arose from a rib more than ordinary, and that it was to that portion attached to the ninth rib that the accessory slip was united. Cast, No. 7.

A detached portion of the *pectoralis major* in another subject took the following remarkable course. It came off from the fifth and sixth ribs, immediately behind the *pectoralis major*, to which it sent several small fasciculi, and with the axillary border of which it was confounded. Coursing across the axilla behind and below the *pectoralis major*, it at length reached the bicipital groove, where the muscular fibres terminated in a flat arched tendon, which immediately divided into three portions. One of these united with the tendon of the *pectoralis major*, at its insertion into the anterior bicipital ridge. Another ascended for a short distance, and then subdividing into several small slips, was inserted into the greater and lesser tuberosities of the humerus, and into the upper part of the bicipital ridges. The third portion passed downwards and inwards over the biceps flexor cubiti, the brachial artery, and median nerve, and behind the basilic vein and internal cutaneous nerve, became slightly confounded with the aponeurosis of the arm, and was ultimately inserted into the internal condyle of the humerus. It was connected with the internal intermuscular septum, immediately after it had passed over the brachial artery and the median nerve, and the two united produced a semilunar border, the concavity of which looked upwards and embraced the artery and nerve. A great number of large fasciculi of the internal head of the biceps extensor, which was much larger than usual, arose from the tendon, from the point of union between it and the internal intermuscular septum to its attachment to the internal condyle of the humerus.* Cast, No. 8.

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* Loc. cit.

Both this and the preceding anomaly altered the relations of the parts connected with the surgical anatomy of the axilla, and there is no doubt that the former, under certain circumstances, must have impeded the circulation through the limb. The lower tendon, or at least a part of it, represents the humeral foramen, seen in the carnivora and sometimes in man.

Musc. Serratus Magnus.—This muscle in one subject presented a marked deficiency of some of the middle fibres, whereby the upper and lower portions of the muscle were separated from each other by a considerable interval. The portion deficient was that normally attached to the fourth and fifth ribs. The deficiency of the digitations attached to the ninth and tenth ribs was observed in two subjects on both sides of the body.

Musc. Biceps Flexor Cubiti.—Strange as the statement may seem, it was by no means an uncommon thing to meet with a biceps having three heads. Thiele † remarks that a third head is found once in every eight or nine subjects, but this does not coincide with my observations which give the proportion of one in fifteen. All the examples of it that I have seen have been remarkably regular in their attachments, considering the variations to which it appears liable, according to the observations of the continental anatomists.

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* This variety is noticed by Thiele, Encyclopedia Anatomie, vol. iii. p. 302. By Cruveilhier, Anatomie Descriptive, translated by Madden, vol. i. note, p. 285, in a modified form.

† Op. cit. vol. iii. p. 217.

The third head in every instance arose from the middle of the humerus on the outer side of the *coraco-brachialis* muscle, between its insertion and the origin of the inner head of the *brachialis anticus.*[*] It was usually broad, thick, and fleshy, but seldom equalled the other heads in bulk, although in more than one instance it was so large as nearly to equal the two normal heads combined. Passing down the arm, it united with one of the normal heads, sometimes with the long, sometimes with the short head, and the three combined were inserted in the usual manner. Cast, No. 9.

In one subject an abnormal muscle was met with, which appearing to me to be a modification of the anomaly just described, I shall describe in this place. The muscle arose from the same place as the third head of the biceps sometimes does, namely, from the outer border of the *coraco-brachial* ridge on the humerus. It is broad and thick, but did not unite with either of the heads of the biceps. Descending to the fore-arm, it was inserted into the aponeurosis of the fore-arm by means of a thick tendon, which crossed over the brachial artery and median nerve about half an inch above the tendon of the biceps, and which, before blending with the antibrachial aponeurosis, had spread out into a thin semilunar band, similar to and above that of the biceps, which was of its usual size and in its normal position. Although this muscle was distinct from the biceps throughout its whole extent, yet its attachments proved it to be a third head of that muscle. The upper attachment, as before stated, was that of the third head; and the lower, although different from that usually seen, yet coincided with one of the normal insertions of the biceps. The anomaly represents that unfrequent variety in the biceps where the long and short heads, instead of uniting, pass separately to their insertion. Cast, No. 10.

It may not be uninteresting to mention, that when a third head exists it is supplied by a distinct twig from the musculo-cutaneous nerve. The muscle just described received the twig supplied to a third of the biceps, thus establishing a further analogy between them. And it may further be mentioned, that the anomaly is a repetition in the arm of the small head which properly belongs to the *biceps flexor cruris*, and that it represents, in some respects, the normal state of the muscle in birds.[†]

Meckel has remarked that the biceps sometimes has as many as five heads, and this anomaly, though somewhat different from that noticed by him, was observed in both arms of a subject remarkable for the number of irregularities in the muscular system. The two normal heads were in every respect regular. The third head, though somewhat smaller than usual, arose from the same part of the humerus as those already described, and terminated in the short head a little above the bend of the elbow. The fourth head arose by a long thin tendon from the capsular ligament of the shoulder-joint, and descending the arm between the long and the short head, blended its fibres partly with the one, partly with the other. The fifth head was of smaller size than the rest; it arose from the outer surface of the humerus beneath the deltoid, and between the long head of the biceps and the outer head of the triceps, and soon became lost in the long head. Each of these heads received a twig from the muscular division of the musculo-cutaneous nerve, with the exception of the fifth, the fibres of which were supplied with nerves immediately after they had joined those of the long head. Cast, No. 11.

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[*] Since this paragraph was written, I have observed the third head arising on the inner side of the *coraco-brachialis*.

[†] Meckel, op. cit. muscle biceps brachial.

Palmaris longus.—This is one of the most irregular muscles in the human body, and consequently numerous abnormal conditions of it were observed. The most common of these was its total deficiency. It was deficient in both extremities once in every three subjects, and in a considerable moiety of the remainder it was deficient in one or other of the extremities. In some of the subjects, in which the muscle could not be said to exist, its place was filled up by slips from other muscles. The *flexor sublimis digitorum* often detached a slip of muscle, sometimes of large, sometimes of small size, to which a thin, long tendon passed on to the annular ligament and palmar aponeurosis. This, indeed, was the most frequent way in which the *palmaris longus* was replaced, but not the only one. Sometimes the *flexor carpi ulnaris* detached the slip of muscle and tendon; and sometimes, but more rarely, this was done by the *flexor carpi radialis.* The most remarkable arrangement, however, was the following:—A small tendon was attached to the outer surface of the anti-brachial aponeurosis about midway down the fore-arm; to this a fusiform mass of muscle succeeded, which again becoming tendinous as it approached the annular ligament, was fixed to the palmar aponeurosis. This was seen only in one subject, but in both arms. Cast, No. 12.

In some few instances where the muscle was more highly developed than usual, it exhibited a tendency to divide in the longitudinal direction into two. This was evinced by the tendon being double, one portion being attached normally, the other being fixed either into the *os pisiforme*, or blended with a part of the tendon of the *extensor ossis metacarpi pollicis.* In one instance the separation was complete throughout the whole extent of the muscle, a process of the fascia of the fore-arm intervening between them above; both muscles, however, were inserted into the annular ligament and palmar aponeurosis.

Another subject presented the *palmaris longus* divided into three separate muscles. The central muscle, the largest of the three, passed into the annular ligament; the internal one was inserted into the *palmar aponeurosis*, and the external one into the base of the metacarpal bone of the thumb.

Flexor sublimis digitorum.—Besides occasionally replacing the *palmaris longus* when absent, and sending slips to the deep flexors of the fingers, this muscle on three occasions was found to be deficient in a slight degree, namely, in the tendon to the little finger; it was not, however, replaced by the *flexor profundus*, as noticed by many anatomists.

Many other minor anomalies of this muscle were observed, but require no mention.

Flexor longus pollicis.—An anomaly of this muscle very frequently observed was the existence of a second head, if the term may be used, fixed to the internal condyle of the humerus, which was sometimes fleshy at one end and tendinous at the other, or tendinous at both ends and fleshy at the middle, and sometimes altogether tendinous. Another not unfrequent anomaly was the receipt of fibres from the *flexor profundis digitorum*, and from the *flexor sublimis* also. In one subject this muscle appeared to arise by three distinct heads, each of considerable size. The first of these was the normal one; the second came from the internal condyle of the humerus; and the third consisted of a short, thick, fusiform muscular bundle, which came from the posterior surface of the *flexor sublimis*, and joining with the second head, united with the first. The appearance and arrangement of the muscle in this subject was very remarkable. Cast, No. 13.

Pronator quadratus.—In one subject the *pronator quadratus* not only had its usual attachments, but also sent off a considerable fasciculus from its lower border, which ran over the carpus behind the flexor tendons, and terminated in a tendon to which a considerable number of the fibres of the deep head of the *flexor brevis pollicis manus* were attached.

Another subject presented the muscle divided into two in the direction of its fibres. The superior portion was attached normally to the radius and the ulna, but the lower portion arising from the ulna, coursed under the annular ligament, where it became tendinous. The tendon, on gaining the palm of the hand, passed between the tendon of the long flexor and the deep head of the short flexor of the thumb, a bursa intervening, and was inserted into the base of the metacarpal bone of the thumb. There can be little doubt that this abnormal fasciculus must have been a very efficient adductor of the thumb.

Extensor Carpi Radialis Brevior.—This muscle occasionally exhibited a tendency to split in its longitudinal direction, the tendon being double. In those instances the two tendons were both inserted into the metacarpal bone of the third or middle finger, the one a little in advance of the other. In one subject this tendency was carried to a much greater extent, so much so as to separate nearly the whole of the muscle into two unequal parts.

Extensor Communis Digitorum.—The anomalies of this muscle were very numerous, and all related to the number of tendons supplied to the fingers, there being sometimes a deficiency, but more frequently a redundancy. Whenever a tendon was deficient, it was invariably found to be that normally furnished to the little finger, and was in some cases partially replaced by fasciculi detached from the *flexor carpi ulnaris*, or by the larger size or even duplicity of the proper extensor of that finger.

When supernumerary tendons existed, it appeared to be in consequence of the splitting of the innermost tendon, and sometimes even part of the muscle; the supernumerary tendon being united to the extensor tendon of the ring finger, and sometimes sending a slip to the extensor tendon of the middle finger, in which case the tendons on the back of the hand were doubled, as they are on the foot.

Extensor Minimi Digiti.—The *extensor minimi digiti* was occasionally deficient, and in those cases was replaced by an additional tendon from the *extensor communis*, or more rarely by fasciculi from the *flexor carpi ulnaris*. More frequently, however, this muscle presented two tendons, or was even divided into two completely distinct muscles, excepting that they were not separated from each other by an aponeurotic septum. In either case the insertion of the tendons was the same, one of them passing to the little finger and uniting with the tendon of the common extensor, the other being especially devoted to the ring finger. This anomaly is stated by Meckel to establish an analogy with many of the mammalia.

Extensor ossis Metacarpi pollicis.—This muscle generally presents a tendency to divide into two parts near its insertion, one part of its tendon being inserted into the base of the metacarpal bone of the thumb, while the other is continuous with the fibres of the short flexor of the same bone; an arrangement which, from its frequency, I believe, with many anatomists, is the normal mode of insertion of the muscle.

This division in the tendon sometimes extended throughout its whole length, and sometimes throughout the muscular portion, so as to divide it into two muscles, the tendon of the innermost being inserted into the base of the metacarpal bone of the thumb, and that of the outermost into the *flexor ossis metacarpi pollicis.* A simple modification of this was observed in one or two subjects, the tendons differing somewhat from the preceding in their mode of attachment. In these the innermost tendon was inserted normally, but the outermost diverging from the other at the wrist joint, passing superficial to the muscles of the thumb, and gaining the palm of the hand, was lost in the outer border of the *palmar fascia.*[*]

Extensor Indicis.—A very common anomaly of this muscle was its division into two distinct muscles, one of which passed to the index finger and united with the common extensor tendon, while the other went to the middle finger, which was therefore provided with a proper extensor muscle. The muscle destined for the middle finger was always smaller than the proper *extensor indicis.*

This irregularity was the most complete one met with; many minor grades of it were noticed, which owe their interest chiefly to the circumstance that they show the exact nature of the higher anomaly. These irregularities were similar to those already described in the case of the other extensors of the fingers, and were simply division of the tendon into two, both of which passed to the index finger, or one to the index, the other to the middle finger; or, again, partial division of the belly of the muscle as well as the tendon.

The proper extensor of the middle finger is, therefore, in all subjects in which it exists, merely an offset from the proper extensor of the index finger. The muscle, on such occasions, may have an attachment to either of the bones of the fore arm, that most usual being to the ulna. It was seen once to arise from the radius.[†] This proper extensor of the middle finger was seen to subdivide into two, the second passing to the ring finger, so that each finger in the hand had a proper extensor, and the arrangement of the extensor tendons in the hand became nearly the same as that of the extensor tendons in the foot.—Casts, Nos. 14, 15.

These anomalies are interesting, inasmuch as they represent the normal state of the muscle in many of the mammalia, especially the apes.[‡]

Adductor Pollicis.—This muscle in one in every thirty subjects was divided into two unequal portions. The superior division was attached to the *os magnum,* and also to the base of the metacarpal bone of the third finger. The inferior division quite distinct, and removed at a distance from the superior, arose from the distal extremities of the third, fourth, and, in two instances, from the fifth metacarpal bones, passed across the palm of the hand in

[*] This anomaly resembles the normal state in the beavers, which have the muscle double.

[†] Meckel has also seen this attachment to the radius, and Soemmering mentions it.

[‡] The opossum, the seals, the loris, and the makis, also present a more or less complete division of the *extensor indicis* into two, one part going to the index finger, the other to the middle finger.

a transverse direction, and was inserted into the internal sesamoid bone on the first phalanx of the thumb. So complete was the division of this muscle, and so great was the space intervening between the two portions, that they appeared to be separate and distinct muscles, the one being the proper adductor of the thumb, the other a *transversus manus*, the analogue, as it most assuredly is, of the *transversus pedis*.—Cast, No. 16.

That the *transversus manus*, where it exists, is a mere offset of the *adductor pollicis* was shown by the muscle in one instance, arising near the centre of the metacarpal bones; and in another by its being separated from the *adductor pollicis* by a very slight cellular interval.

Flexor brevis minimi digiti.—The short flexor of the little finger arose in one subject by two distinct heads. The first or short head was the normal one, and arose from the unciform bone, as usual. The second or long head was situated principally in the fore arm. It arose from the commencement of the lower third of the inner border of the ulna, by means of a short tendon, to which a long fusiform muscular belly succeeded. This belly coursed over the *flexor carpi ulnaris*, the ulnar artery and nerve, passed beneath the annular ligament, and then turning outwards, united with the short head midway between the unciform bone and the first phalanx.—Cast, No. 17.

Palmaris brevis.—This muscle is said to be frequently deficient, but I believe erroneously. Whenever it was looked for, it was invariably found, although not so highly developed in some cases as in others, for it often consisted of but one or two small fasciculi of fibres mixed up with and concealed by the fatty tissue of the hypothenar eminence.

Triangularis sterni.—This was a most irregular muscle. In several subjects the whole muscle was deficient on one and sometimes on both sides. In others the upper part was wanting, and in a few the lower fasciculi could not be found. A large proportion of the subjects, about one in four, presented the usual tendinous attachment between it and the *transversalis abdominis*, transformed into muscle, so that the fibres of the two were directly continuous, and no line of demarcation could be drawn between them. So common is this anomaly, that it has been proposed to call this muscle with the *transversalis abdominis* the *sterno-abdominalis*,[*] but this could not be permitted, since the connection between them must be considered an irregularity.

The irregularity shows that the *triangularis sterni* is a repetition of the *transversalis abdominis* in the chest, and that the usual mode of describing its attachments should be reversed.

[*] Rosenmuller. De nonnullis musc. corp. hum. varietatibus.

Obliquus internus abdominis.—Complete or incomplete tendinous intersections of this muscle were not unfrequently observed. In one subject, in which they were remarkably well developed on both sides,[*] there were three of them, one opposite the ninth rib, another opposite the eleventh, and the third opposite the twelfth. They were all continuous with the anterior extremities of these ribs.

These intersections afford additional evidence in favour of the very general opinion that the internal oblique muscle corresponds to, and is a repetition of, the internal intercostal muscles, since the intersections are representatives of abdominal ribs.

Pyramidalis.—This muscle was deficient on both sides once in every four subjects,[†] and was wanting on one side in a moiety of the remainder. In these cases the *rectus abdominis* was always broader on one or both sides. One muscle was found to be always longer than the other, and this was most frequently the case with the right. It was observed once to be double on both sides, but all four muscles were very small, although distinct.[‡]

Psoas Parvus.—The most contradictory statements are made with regard to the frequency of anomalies of this muscle. Thus Soemmering[§] states that it is often absent; Thiele[‖] believes that its absence should be considered the normal state; and Meckel[¶] remarks, on the contrary, that it is rarely wanting. I have carefully examined all the subjects dissected in the rooms, and found that the *psoas parvus* was more frequently present than absent, although the proportion was very nearly as one to one, the numbers being 61 to 54.

The sex of the subject has been presumed to exercise an influence over the presence of this muscle, but here the most opposite statements have been made. Winslow[**] and John Bell[††] both state that they have met with it more frequently in females than in males; whilst Riolan[‡‡] asserts that he has never seen it in females. My own observations show that the sex has nothing whatever to do with its presence or absence, for it was as often deficient in the female as in the male.

Pyriformis.—About one in every twenty subjects presented this muscle divided into two unequal portions, a superior and an inferior, between which the peroneal division of the great sciatic nerve passed. In one subject it was divided into three portions, the sciatic nerve being also divided into three fasciculi, two of which passed between the divisions of the pyriformis.

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[*] Soemmering states that he has only met with them on the left side in man. Op. cit. p. 146.

[†] This muscle is generally considered to be rarely abnormal.

[‡] This is a very rare anomaly; it is mentioned by Sabatier, Traité complet d'Anatomie, Vol. i. p. 263.

[§] Op. cit. p. 291. [‖] Op. cit. p. 284. [¶] Op. cit. m. petit psoas.

[**] Exposition anat. struct. corp. hum., Vol. ii. p. 211.

[††] Anatomy of the bones, p. 341. [‡‡] Anthropographia, Vol. v. p. 508.

I may mention here, that the origin of this muscle from the upper part of the sciatic notch (posterior inferior spinous process of the ilium) has been described as an anomaly. If it were such, I should have to state that it was frequently met with, but I believe with the greater number of anatomists that this attachment is normal, it being more common to find it present than absent.* This portion of the muscle was sometimes unusally large, and was, in such instances, generally united with the *glutæus medius*, and then the two appeared to be one muscle.

Gemelli.—The superior gemellus was often found deficient; and, more rarely, was of larger size than usual. The inferior gemellus was observed to be deficient on two occasions. It was more frequently increased in size. The former of these anomalies is stated by Vicq d'Azyr,† to establish an analogy with the apes.

Quadratus Femoris.—This muscle, which is generally supposed to be often deficient, was only found so once; and, in that instance, was associated with an unusual development of the two gemelli and the *obturator internus*. It may be interesting to remark that this anomaly represents the normal state in the Cheiroptera and in the Myrmecophaga.—Cast, No. 18.

Sartorius.—The sartorius was observed to have a tendency to divide into two muscles in the direction of its length on two different occasions. On one of these, the lower half of the muscle was divided into two parts, an anterior and a posterior, both of which were inserted into the inner tuberosity of the tibia, one above the other. The other was a modification of this, the posterior portion being inserted normally, while the anterior was inserted into the *fascia lata*, over the lower part of the *vastus internus*, and into the *ligamentum patellæ*. It may be stated that the muscle in both instances was unusually broad, and that the long saphenus nerve passed from the thigh into the leg between the two portions of the muscle.

Biceps Flexor Cruris.—No anomalies proper to this muscle were noticed, but the following supernumerary muscle having a connection with it, I have thought proper to describe it in this place. The muscle in question consisted of a band of fibres about three quarters of an inch broad, which passed from the middle part of the short head of the *biceps flexor cruris*, over the commencement of the middle third of the popliteal artery and vein, between them and the popliteal near to the inner and posterior side of the rounded tendon of the *adductor magnus*, to which it was attached. The relation of this muscle to the popliteal vessels is interesting, in consequence of its passing over and concealing that part of the artery to which a ligature is generally applied.

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* This attachment was seen in nine out of every ten subjects.

† Ency. meth. syst. des quadrup., p. 29.

Peroneus tertius.—This muscle is one of those which present every gradation of size, being sometimes of great strength, at other times altogether absent. The only instance I have met with in which the latter anomaly existed, was in a subject remarkable for the great number of anomalies by excess in the muscular system, amongst which were a three-headed biceps, a *sternalis brutorum*, a double extensor of the metacarpal bone of the thumb, and a proper extensor for the middle and another for the ring finger. I mention this circumstance because I shall have to refer to it hereafter. Thiele,[*] in noticing an anomaly of this muscle, remarks, that the muscle seems sometimes wanting, because we do not perceive a special tendon for the posterior part of the metatarsus; and, moreover, that the muscular fibres are then not less dispersed than ordinary, only they unite with the external tendons of the long common extensor of the toes;—a remark which applies not only to deficiency of the muscle, but in a minor degree to all other anomalies of this muscle.—Cast, No. 19.

Peroneus Brevis.—Before describing the anomalies of this muscle, it will be necessary for me to make a few remarks concerning what I conceive to be a part of the normal insertion of this muscle. Thiele[†] has enumerated, amongst the anomalies of the *peroneus brevis*, a tendinous slip passing from behind forwards along the dorsum of the foot, covered by the tendon of the third peroneal muscle, to gain the little toe. Now this slip of tendon, detached from that of the *peroneus brevis*, I have found in a modified form in four out of every five subjects; if, therefore, we are to take the comparative frequency of its presence as evidence, it must be allowed to be normal. The modification to which I allude is, that it does not pass into the toe itself, but is united to the outermost tendon of *extensor longus digitorum pedis* over the metarso-phalangeal articulation.[‡]

This tendinous slip was observed to be occasionally converted into muscle at its posterior part, and was then found to be of larger size than ordinary. In one instance the tendinous slip came off from the *peroneus brevis* near the middle third of the leg, and presented in its centre a long fusiform muscular belly, which extended from opposite the ankle joint to the middle of the fifth metatarsal bone, the tendon terminating as usual in the long extensor tendon of the little toe. In another instance of the anomaly, the muscular portion commenced directly from the tendon of the *peroneus longus* at the ankle joint, and, proceeding forwards for about two inches, gave rise to the tendon, which terminated in the ordinary manner.—Cast, Nos. 20, 21.

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[*] Op. cit. p. 311.

[†] Op. cit. p. 326.

[‡] I find in my note-book the following extract, but from what work I took it I cannot say, but I believe it to be from Meckel's Anatomie Compàree. "Chez l'homme, le tendon du court peronier se divise *communement* en deux, dout l'un se fixè à la tubèrositè du cinquieme mètatarsien, l'autre au bord externe du quatrième tendon du long extenseur des orteils et au quatrième muscle interosseux superieur." A remark which closely coincides with my own observations.

In more than one subject the tendinous slip was seen to divide into three distinct portions; one portion being inserted into the distal extremity of the fourth metatarsal bone; another into the proximal extremity of the fifth metatarsal bone; and the third into the long extensor tendon of the little toe.

It is interesting to observe, that the development of the supernumerary muscle described above causes the *peroneus brevis* muscle in the human body to resemble in some degree the normal state of the muscle in the bear, where it consists of two portions, one passing into the fifth metatarsal bone, the other into the extensor tendon of the little toe.*

Plantaris.—In two subjects the following anomaly of the plantaris muscle was observed. The muscle arose from the femur in the ordinary manner, and then descended for about an inch and a half, where the tendon commenced. The tendon departed from the normal state, for, instead of being a long, thin, and slender tendon, it spread out into a broad aponeurosis about three inches in length, which attached itself to the tendon of the gastrocnemius muscle a little above where its tendon unites with that of the soleus to produce the *tendo Achillis.*

Most anatomists, following Meckel, have stated that the plantaris muscle is very frequently deficient, and that it is more often so than its analogue the *plamaris longus*. Now, strange as it may appear, not one subject out of the whole one hundred and five dissected wanted this muscle either on one or both sides, while, as already shown, the *palmaris longus* was often absent. I am therefore inclined to believe that Meckel† must have been mistaken, and that Gantzer‡ was correct when he stated that the plantaris was more constant than the *palmaris longus*, although Meckel§ contradicts the statement in a note.

Soleus.—A double soleus muscle was observed in a subject remarkable for the development of the muscular system. The ordinary soleus muscle was attached in the usual manner, and the only peculiarity noticed was a diminution in point of thickness. The duplicate muscle separated throughout its whole extent from its fellow by means of a large quantity of adipose tissue, arose from the tibia and fibula, and from the deep fascia of the leg by the ordinary attachments of the soleus. The fleshy portion of this abnormal muscle was well developed and was much larger than the belly of the muscle superimposed on it. The tendon proceeding from its lower extremity was entirely unconnected with the *Tendo Achillis*, was broad, flat, and firm, and was inserted into the upper and inner part of the posterior tuberosity of the *os calcis*, about half an inch anterior to the place of attachment of the *Tendo Achillis.*

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* The anomaly also gives the little toe a proper extensor, and causes the hand and foot to resemble each other more closely.

† Op. cit. muscle plantaire grele.

‡ Dissertatio Anatomica Musculorum Varietates Sistens, p. 4.

§ Op. cit. ibid.

A modification of this curious anomaly was met with in two other subjects, the modification resulting from the incomplete separation of the two muscles at their upper part. The duplicate muscle had the same attachments as in the preceding subject, with this addition, the greater number of the superficial fibres of the upper half of the muscle were attached to the deep aponeurosis of the normal soleus. The attachment to the *os calcis* was also slightly different, the tendon being inserted more internally into the posterior tuberosity of that bone. Cast, No. 22.

The duplicate muscle, although not possessing any actual representative in the lower animals, yet seems to have some relation to the normal state of the muscle in the bears and amongst the ferrets, which have the soleus entirely separate from the *Tendo Achillis* and inserted by a tendon distinct from the gastrocnemius into the *os calcis.*

Flexor accessorius longus digitorum pedis.—I thus denominate an anomalous muscle occasionally met with seated deeply amongst the structures at the ankle joint, the back of the leg and the sole of the foot. It was observed to be remarkably regular as regards its general characters, so that the description of one will serve to point out the appearance, direction, and attachments of this muscle.

It arose by a small pointed tendon from the posterior border of the fibula between the *peroneus brevis* and the *flexor longus pollicis pedis* about two inches above the ankle joint. The small tendon soon after expanded into a somewhat broad aponeurosis, which afforded attachment to a considerable number of muscular fibres. These fibres were all connected with the internal surface of the aponeurosis and were soon collected into a long fusiform mass, which passed obliquely inwards and downwards over the peroneal artery to the ankle joint, and then curving forwards over the inner side of the *os calcis*, where it was placed between the posterior tibial vessels and the tendon of the long flexor of the toes, and superficial to the vessels reached the foot. As soon as it had gained the sole of the foot, it terminated in a long rounded tendon, which ultimately attached itself to the tendon of the long flexor of the toes. The *flexor accessorius*, which I have remarked, is more than usually large where this muscle exists, was attached to its tendon before it united with the long flexor.

This muscle was observed to terminate in one subject at the ankle joint, by joining with the long flexor of the toes, but the mode of attachment most frequently met with is that described above.

Transversus Pedis.—This muscle, which, as we have already seen, has no analogue in the hand, except when the *adductor pollicis manus* is abnormally divided into two, often causes the muscles of the thumb and of the great toe to correspond by being deficient. The nature and mode of origin of the "*transversus manus*," as pointed out in a former part of this notice, would appear to indicate that the "*transversus pedis*" is properly an offset of the *adductor pollicis pedis.*

Besides the irregularities just described, many others were noticed, especially amongst the deep-seated muscles of the back; these, however, I have not thought it necessary to mention, since they were for the most part mere variations, dependent on the number of spinous or transverse processes of the vertebræ to which they were attached, and these so variable that it would be difficult to state what was normal what abnormal.

General Remarks.—In the preceding pages a great number of anomalies of the muscular system have been described, many of them being exceedingly common, and some being apparently very rare, yet, in all probability, they do not constitute one tithe part of the irregularities to which the muscular system is liable. Indeed, so common are these irregularities, and so frequently are some of them met with, that a subject in which the muscular system does not deviate in one or more of its parts from what we are pleased to term the normal state is very rarely seen. If we leave out of consideration the *psoas parvus* and the *palmaris longus*, one or other of which are almost always absent where no other anomaly exists, the relation of the normal state to the abnormal would be, according to my own observations, as one to two, or, in other words, only one subject in three would present the muscular system in a perfect state—a circumstance which alone renders an inquiry into the nature of the irregularities one of deep interest.

It is a very uncommon thing to meet with but one irregularity of the muscular system in a subject. Most generally there are three or four of them situated in different parts of the body; and occasionally a much larger number are found. One subject presented no less than seventeen, namely, one in the neck, three in the trunk, eight in the upper extremity, and the remainder in the lower. The general rule, therefore, would appear to be that two or more irregularities in the muscular systems are associated together in the same individual.

That the muscles in the different regions of the human body are not equally liable to deviate from the normal type will have been noticed during the perusal of the individual anomalies. The muscles of the trunk, excepting those of the back and the *psoas parvus*, appear the least variable, next those of the head and neck, then those of the inferior extremity, and, lastly, the muscles of the superior extremity, the irregularities of which are very numerous and very common. A more attentive examination of them will also show that the conclusions drawn by M'Whinnie* are correct, namely, that those muscles whose functions are the most uniform throughout the animal kingdom are the least liable to deviate, as for instance the muscles of mastication, deglutition, &c. while those which enable the extremities to perform actions peculiar to the different species of animals—as prehension, flying, swimming, burrowing, and so on—constantly vary, amongst which we may mention the *pectoralis major*, the biceps, and the extensor muscles of the fingers in the upper extremity; and the external rotator muscles in the lower. The reason why the muscles of the lower extremity are not so liable to deviate as those of the upper, irrespective of number, is also explained by the law just established, since the former do not undergo such extensive changes amongst the lower animals as the latter;—an explanation which is greatly supported by the difference in the frequency of anomalies in the vascular system and in the osseous system also in the two extremities.

Anomalies of the muscular system may result from many different causes, those of which we have to speak at present being, displacement of one or other extremity of a muscle, an augmentation or a diminution in the number of the muscles, or of the component parts of the muscles, all of which may, in some few instances, be met with in the same individual. Thus, in the subject already mentioned, in which there were no less than seventeen different anomalies, there was one by displacement, namely, the attachment of the sterno-thyroideus to the clavicle and not to the sternum; four by defect, viz. the deficiency of the *palmaris longus*, the *psoas parvus*, the *peroneus tertius*, and the *transversus pedis;* the rest were all anomalies, consisting in an increase of the usual number of muscles, or in an augmentation of the parts of muscles. The association of anomalies by defect with anomalies by redundance in the same individual is, however, not confined to the muscular only, but may be observed in numerous instances of the higher deviations, as in monsters with eventration and in double monsters, in all the other systems of the body. This, as I have pointed out elsewhere,† is one of the most paradoxical as well as remarkable circumstances connected with the history of aberrations from the normal state. In the case of the muscular system, there can be no doubt that they result from one and the same cause, namely, some vicious conformation of the ovum; and there is no reason why this should not be the case with all other irregularities, whether slight or grave. I believe it to be an unrefutable argument in favour of the original production of some monsters, which are now generally supposed to arise from mechanical causes supervening during development, although those who believe it cannot produce sufficient proofs in its favour.

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* Loc. cit. p. 184.

† Observations illustrating the Anatomical Structure and Physiological History of Monsters with Eventration.—Edin. Med. and Surg. Journal, vol. lxviii. p. 303.

These anomalies may be either symmetrical or unsymmetrical, that is, may be present on both sides or only on one side of the body. It will have been noticed, that, in describing the anomalies, I have not always stated whether they are single or double. I considered it unnecessary to do so in all cases, and I therefore state here that those which have come under my notice have been more frequently symmetrical than unsymmetrical. It may be remarked, however, that some anatomists have found the reverse to be the rule.

Displacement of the muscles is by no means of frequent occurrence, and is, as far as my observations will permit me to state, principally confined to those in the neck. The most remarkable, because the most unlooked for of these, is the displacement of the lower attachment of the sterno-thyroid, but the most important is that of the posterior belly of the omo-hyoid. It is impossible to explain how the displacement should occur in the case of the sterno-thyroid; but in that of the omo-hyoid we are enabled to trace it, in the first instance, not to any fault in the muscle, but to the cervical fascia, which, as already shown, often binds the muscle closely down to the clavicle.

Deficiency in the number of parts of muscles or in the number of muscles themselves is of common occurrence. The deficiency is usually confined to a certain class of muscles whose use in the economy of the human body is very slight, and which may be dispensed with without any detriment to the function of the parts in which they are usually found, or be easily performed by others; we need only instance the *palmaris longus*, the *pyramidalis*, the *extensor minimi digiti*, and the *gemellus superior* in support of this statement. Some of these anomalies by defect appear to result from the fusion of the deficient muscle with that to which it has the closest relation. If I am correct in my supposition that the *levator glandulæ thyroideæ* is a double muscle, of which the two parts are sometimes separate, sometimes united, it would afford us one of the most striking proofs in favour of the hypothesis. That the deficiency of the *peroneus tertius* arises from this cause is very apparent, inasmuch as the muscular fibres to which its tendon is usually attached are not deficient, but unite with the outermost tendon of the *extensor communis digitorum pedis*, as remarked by Thiele. The same may be said of the *pyramidalis*, which is replaced by the *rectus*. But this does not apply to all, as, for instance, to deficiency of the *palmaris longus* and of the *psoas parvus*, of which no other explanation can be given than that the germs of these muscles are not produced during development.

Increase in the number of the muscles or of the parts of muscles is also of frequent occurrence. In the generality of instances, it is accompanied by a more marked development of the whole muscular system, but that it is not dependent on this is proved by the fact, that supernumerary muscles or slips of muscle are also met with in thin subjects. The manner in which the number of muscles or slips of muscles is augmented appears to originate in various ways.

1. The splitting of a muscle in the longitudinal direction may give rise to it. This is clearly demonstrated by numerous instances recorded in the preceding pages. The doubling of the *extensor ossis metacarpi pollicis* and the production of a proper extensor of the middle finger are good examples. The first stage is generally a mere division of the tendon, which may be normal. To this succeeds a partial division of the muscle, which, when carried to its full extent, causes complete division of the muscle into two, a distinct cellular interval intervening between them. These stages have been observed not in the same, but in different subjects. Amongst the supernumerary or double muscles which originate in this manner may be mentioned the quadruple geniohyoids and pyramidales, the double sartorius, the double and treble palmaris longus, the " transversus manus," and many others.

2. The augmentation many originate in a transverse splitting of the muscle, or, in other words, by a division in the direction of the thickness of the muscle. That a muscle may be doubled in this manner appears certain from the observations I have made on the irregularities of the solæus. The stages in this process appear to be the same as mentioned for the preceding. The double poplitæus seen by Fabricius,* the double *pectoralis major* and *glutæus maximus* noticed by Tiedemann,† and some few others of the same kind, are examples of double muscles originating in this manner. It seems probable that the slips of muscle passing across the axilla, which we have described, originate in a modification of the same action, a small portion of the muscle being separated wholly or partially from the rest, and having attachments somewhat different from those which the portion of muscle would have had, had the muscle been normal.

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* Ab. Aquapendente. De motu locali animalium. In operæ, p. 359.

† Deutsches Archives für die Physiologie, vol. iv. p. 412.

3. Another portion of the supernumerary muscles arise from the development of muscular fibres in normal tendinous structures; those tendinous structures being at the same time more highly developed than ordinary. This is not a common mode of origin of supernumerary muscles, although we might naturally have expected to meet with it very often. The most common examples of it are in the third belly of the digastricus, which is evidently a development of muscular fibre within, and in the place of, the supra-hyoid aponeurosis, and in the proper extensor of the little toe, which, as already shown, consists in the development of muscle in the upper and posterior part, or in the centre of the slip of tendon sent off from the *peroneus brevis* to the long extensor tendon of the little toe.

4. Another mode of origin of supernumerary muscles is to be found in the development of the cuticular muscle, of which the *platysma myoides* and the *palmaris brevis* are the only remains to be seen in man. The cuticular muscle here referred to is the *panniculus carnosus* of animals, and one of the muscles which I presume to originate in a much higher development of it in man than ordinary, is the *rectus sternalis* or *sternalis brutorum.* If we directed our attention to the *sternalis brutorum* only in its highest degree of development, it would be difficult to discover its relation in point of position to the integument, and therefore to the *panniculus carnosus ;* but if we trace it through its various grades of size from the small indistinct trace of muscular fibre to the large, robust, and firmly attached muscle, it will be seen that it belongs to the same class of muscles as the *platysma myoides* and the *palmaris brevis*, and is consequently a development of the cuticular muscle, of which but a small portion can be traced in man. The muscle in its imperfect, as well as in its perfect state, is always situated superficial to the *pectoralis major* and its proper cellular covering, and, moreover, in that state is not attached to the bones, but its two extremities are either blended with the fibres of the *pectoralis major*, or are lost in the fascia superimposed on it.

5. The origin of some of the supernumerary muscles cannot be explained by any of the foregoing laws, and can only be referred to the evolution of germs which do not exist in the greater proportion of embryoes. The slips of muscle passing transversely across the axilla, between the tendons of the *latissimus dorsi* and *pectoralis major* do not appear to be offsets from either of these muscles, but to originate independent of both. The same appears to be the case with the long accessory flexor of the toes before described, for, as far as I could determine, it had no connection either real or apparent with the long flexor of the great toe, from which it might have been supposed to proceed. The third,

fourth, and fifth heads of the *biceps cubiti* seem to originate in the same way. Meckel and others have seen the third head consisting of a detached portion of the *brachialis anticus*, so that even when highly developed, and the connection not apparent, it may be considered as an offset of the *brachialis anticus*. The fourth and fifth heads have certainly no connection with any other muscle, and therefore must originate from supernumerary germs.

It is a remarkable circumstance in connection with the development of additional muscles, that, when non-symmetrical, they will be most frequently met with on the right side of the body, inasmuch as in this, as in most other respects, the muscular system coincides with the vascular and osseous systems, and supports the opinion that the right half of the body is more highly developed than the left.

Supernumerary muscles, or parts of muscles, when attached to parts capable of being moved, must necessarily give rise to a greater extent, and it may be freedom of motion, than ordinary. Thus, when the ring finger is provided with a proper extensor, that finger must be more perfectly under command of the will than when it has merely the tendon of the common extensor, and that bound to the tendons of the adjoining fingers. It is even possible that, in some cases, additional strength is given to an individual by the possession of numerous additional heads or parts of muscles. Tiedemann found the *pectoralis major* and *minor*, the *trapezii* and *glutæi maximi* muscles double in the same subject, and inferred from this that great power in man is not always the result of exercise, but sometimes depends on the congenital redundancy of numerous large muscles. Meckel having seen another instance of the same kind, was led to form the same opinion. And I am inclined to support this important physiological deduction, from having noticed that while the smaller muscles may be doubled in spare subjects, the larger muscles are seldom found to have a redundancy of heads except where the whole muscular system is remarkably well developed.

There are many of the supernumerary muscles which are useless, and there are some which may be even injurious. In one instance I noticed that the tendinous slip, prolonged from an offset of the *pectoralis major* to the internal condyle of the humerus, had tied the brachial artery and median nerve so tightly down, as to cause a considerable constriction in both of them. Some of the slips of muscle crossing the axilla appear to cause occasional compression of the axillary artery, as already stated. In both these instances, therefore, there is sufficient reason for supposing that the circulation of the limb was interfered with.

That supernumerary muscles are sometimes of great use to the economy, the following case will, I think, sufficiently testify.

W. S., aged 78, died of apoplexy in December 1846. On examining the body after death, it was found that the greater part of the muscles in the body had been wholly or partially converted into fat. Amongst those which had been completely transformed into fat, where all the muscles of inspiration, without exception, none of them, indeed, could be detected by their colour; the striated fibrous character of some structures composed almost entirely of fat, together with the direction of the striæ, alone indicated their position and previous existence. How, then, did this individual inspire? The *sternalis brutorum* existed in its highest state of development, was robust, and of a deep red colour, contrasting strongly with the pale fatty appearance of all around it. It was this muscle, then, which elevated the ribs on one side of the chest, and increased the capacity of the chest to some extent. Considering the state of the muscular system, there could have been but little demand for oxygen. But whatever the quantity demanded, the *sternalis brutorum*, by contracting, and thereby increasing the capacity of the chest, must alone have caused to enter the lung.*

The abnormal arrangement of the muscles in the two extremities, the pectoral and the pelvic, render their similitude much more perfect than it is when the arrangement is normal. This circumstance, which has been noticed by Meckel and others, causes the muscular system to come under the same general law as the vascular and osseous systems.† The two extremities in their normal state correspond, yet differ. They correspond very closely in the number of parts, and in the arrangement of those parts; but they differ somewhat in detail, in consequence of the difference in their use in the economy. The pectoral extremity being principally adapted for prehension, and being required to execute the most varied movements with the greatest precision and delicacy, has all its parts endowed with the greatest freedom of motion: the pelvic extremity, on the other hand, being required to support the weight of the body in the erect posture, and during progression, has all its parts more solidly constructed.

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* This interesting case will be noticed at full length, and in all its bearings, at some future period,

† The law referred to is a mere repetition of the words employed in the text. It has long been known in regard to the muscular and osseous systems, but has not been fully established until lately in regard to the vascular system. Dr Knox (Engravings of varieties in the origin, course, and distribution of the arteries; fasciculus II. 1836,) pointed out the occurrence of a variety in the division of the axillary artery, which afforded "an additional proof of the close analogy existing between the pectoral and pelvic extremities." This variety has occurred to me during the last year on three several occasions. I have also noticed varieties in the arteries of the foot and hand closely resembling each other, and bringing those parts into more close analogy than they have normally. Indeed, it would not be difficult to show that all the parts of the arterial system in the extremities, when abnormal, are *precisely* similar. The veins conform to the same law (see a Memoir on Anomalies of the Nervous System in the Medical Times for November 1847).

These differences in function and construction of the limbs taken as a whole, necessarily give rise to differences in the arrangement of the individual parts entering into their formation; but these differences are rendered very slight by the numerous and frequent aberrations of the muscles from their normal state. It is needless for me to particularize the anomalies which cause the general arrangement of the muscles in the two extremities to be so similar, as most of them have been noticed whilst describing the individual anomalies, to which I therefore refer.

Is the study of the anomalies of the muscular system of any use to the practical surgeon? I am led to believe that it is; and furthermore, consider that the surgeon ought to be equally conversant with the more important anomalous distributions of the muscles as with anomalous distributions of the vessels. My reasons for believing this are threefold; first, anomalies of the muscles are of more frequent occurrence than those of the arteries; secondly, they often alter most materially the boundaries of regions and the landmarks, if they may so be called, by which the surgeon is accustomed to guide himself in performing operations on the more important parts of the body; and thirdly, they often alter the relations of the larger arteries and other important structures. Whether any difficulties in performing an operation on the living body have been hitherto met with in consequence of an abnormal development or attachment of muscle, is a point I am unable to determine; but there can be little doubt that they may arise, as the following circumstance will clearly show. Some naval surgeons, in pursuance of the regulations of the naval medical board, were engaged during last autumn in performing the usual course of operations on the dead body. They wished to place a ligature on the posterior tibial artery near the centre of the leg, according to the method recommended by Mr Guthrie, but after cutting through the structures usually overlying that vessel, and searching for some time, were unable to find it, and therefore discontinued their efforts. Happening to enter the rooms shortly, and being informed of their failure, I examined the parts, and it having occurred to me that the soleus muscle might be double in this instance, the dissection was carried deeper through a muscle nearly half an inch in thickness, and the artery was ultimately secured. A further dissection was subsequently made, and the soleus was found completely double, as described in the former part of this memoir. The same circumstance MIGHT occur in the living body, and unless the individual operating happened to be acquainted with the fact that such an anomaly sometimes exists, what would be the result? Other regions of the body are equally liable to have their relations altered, and, consequently, I may be allowed to infer, that similar difficulties might be met with in operating on those regions from similar causes.

The continental anatomists have studied the anomalies of the muscular system, because they have been found of some value to physiology, and more especially to that division of the science which has for its purpose the investigation and determination of the laws of formation, both normal and abnormal. I have studied them with the same purpose, but have always considered they might be of some practical value, and consequently have noticed, as far as the comparatively limited number of my observations will permit, the changes in the boundaries and relations of certain regions and parts which result from the abnormal development and attachment of some muscles. In my future notices, I intend to enlarge more particularly on this point, merely desiring for the present to draw attention to the general bearings of the subject.

ART. II.—*Observations on the Climate of Largs, with Notices of the other principal Watering-places on the Clyde, and their general Characteristics.* By WILLIAM DAVIDSON, M. D., formerly Physician to the Glasgow Royal Infirmary.

LARGS.

THE parish of Largs is situated on the Frith of Clyde, along which it extends about nine miles from north to south, and is stated to be in latitude 55° 49′.30 north, longitude 4° 52′ west. It consists chiefly of a stripe of level rich land bordering the sea, narrow at many points, particularly towards the north, with a range of hills behind, one of these being nearly 1700 feet above the level of the sea. The town is situated on a small plain towards the southern end, the distance from the shore to the base of the mountains being from half a mile to a mile. Part of Bute and the whole of the larger island of Cumbray lie on the opposite side of the Frith, while the picturesque and irregular peaks of Arran are seen in the same direction and at a few miles greater distance. The sea view from some of the heights above Largs is magnificent, and many interesting objects arrest the attention, such as the Rock of Ailsa, the Arran mountains, the green island of Cumbray, the peculiar promontory named Port Crawford, and the apparently boundless Atlantic Ocean. Before the establishment of steam-vessels, the parish was rather a sequestered spot, being partially cut off from communication with the interior of Ayrshire by the high land to the east, and by the circuitous road along the coast from the towns of Renfrewshire. It would probably strike a stranger on viewing the town of Largs, its situation on a level plain, with a high range of hills behind, and flanked to the north and south with somewhat elevated ground, while in front was the Frith of Clyde, with the larger Cumbray and the Crawford promontory acting as natural break-waters, that it was well sheltered for a coast residence. And so it is, to a certain extent, defended from the violence of the southern and western gales; but from its position at the base of the hills referred to, which have an elevated inland country behind, it is easy to comprehend, how an east wind from the German Ocean blowing over a considerable tract of land, should descend through the valleys and ravines, as if through flues, into the warmer regions below with considerable velocity. The town proper, as far as the Broomfields or elevated portion of the beach is much exposed during spring to east winds, which blow down the Gogo and Brisbane glens; the Crescent less so, while the villas erected to the north of the Noddle river are almost altogether defended by the wooded banks behind, although they are somewhat open to the north. The houses and villas extend along the shore for about a mile and a half, are generally elegant and tasteful in their architecture, which, combined with the green and beautifully shaped hills on the back ground with the ocean scenery, render the locality extremely beautiful at all seasons of the year. The town proper is very ancient, and consists of a series of streets, several of them narrow, running at right angles to one another or east and west, and north and south. Cottages of moderate dimensions for the summer visitors would enhance its value as a watering-place, as there is rather a deficiency of such accommodation.

Temperature.—As Largs partakes to a considerable extent of an insular climate, the range of heat and cold is less than in the interior of the country; the cold not being so intense in winter or the heat so great in summer, while the diurnal variations are also less. This arises from the prevalence of south and westerly winds blowing from the warm Atlantic. Near the shore ice is rarely formed, unless the thermometer indicates a temperature several degrees below the freezing point; but the small lochs in the recesses of the mountains are more frequently frozen. Snow rarely lies on the beach or ground near it for more than a few hours, unless the temperature be very low, but on the sides of the mountains it is more persistent. Snow storms are rare, but hail frequently accompanies westerly and north-westerly gales of greater or less magnitude.

The following table contains the results of observations made at Largs respecting its temperature, quantity of rain, weight of atmosphere, &c.*

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* The situation was on the shore, nearly on a level with the sea at high water. The thermometers, had an eastern exposure and were furnished with shades, the barometer was hung on the second floor of the house, and the rain-guage was placed in the garden behind, nearly three feet from the ground, but unavoidably sheltered to the west by the house.

METEOROLOGICAL TABLE.—I.

Month.	Therm. 9 A.M.	Therm. 9 P.M.	Mean	Barom. 9 A.M.	Rain Gau.	Wind.								Fair.	Rain	Snow & Hail	Thunder.	Gale	Breeze.	Cloudy.	Reg. Max.	Therm. Min.	Mean
						E.	W.	N.	S.	N.-E.	N.-W.	S.-E.	S.-W.										
1845																							
Dec.	40.9	40.7	40.8	29.75	3.4	5	6	...	5	1	8	...	5	12	18	1	2	10	5	...	...	...	...
1846																							
Jany.	44.3	44.7	44.5	29.7	5.4	4	2	...	8	...	1	7	9	10	19	2	...	2	13	9	...	...	...
Feby.	45.	44.2	44.6	29.94	3.2	...	4	...	5	...	13	5	1	16	12	...	...	4	13	9	...	...	...
March	43.7	42.3	43.	29.74	5.5	2	3	1	9	1	5	8	2	11	15	5	...	2	14	3	..	..	...
April	46.8	45.5	46.1	29.87	2.2	5	2	2	4	8	2	6	1	15	14	1	...	...	13	10	...	...	...
May	54.	51.4	52.7	29.98	1.8	1	10	1	9	2	3	4	1	17	14	...	...	...	13	7	...	...	...
June	63.8	60.6	62.2	30.07	4.3	3	4	...	12	1	2	3	5	22	8	...	...	...	7	11	...	...	...
July	59.9	58.	58.9	29.9	6.2	2	6	...	12	3	1	2	5	10	21	...	1	...	17	4	...	...	...
August	62.1	59.7	60.9	30.05	4.2	5	4	1	9	...	9	1	2	19	12	...	2	...	7	3	...	...	...
Sept.	58.3	57.6	57.9	30.08	3.0	8	3	...	10	...	3	2	4	20	10	...	...	...	14	12	...	..	...
Oct.	50.2	50.5	50.3	29.99	5.1	4	3	6	6	...	4	6	2	15	16	...	...	1	15	10	...	...	...
Nov.	44.7	45.2	44.9	29.96	5.0	6	2	3	5	2	1	11	...	15	14	1	1	...	12	10	49.1	42.1	45.6
Dec.	37.4	36.9	37.1	29.97	1.2	...	1	11	3	5	5	2	4	19	10	2	...	1	10	7	41.5	32.1	36.8
1847																							
Jany.	37.8	38.5	38.1	29.89	3.0	17	...	1	3	2	2	5	1	18	10	3	..	...	14	15	41.3	34.6	37.9
Feby.	37.8	38.1	37.9	29.99	1.3	6	3	5	4	...	6	2	2	17	9	2	...	..	17	2	43.5	32.2	37.8
March	43.3	42.3	42.8	30.1	1.2	2	...	7	6	2	7	4	3	22	9	...	1	..	14	11	49.9	37.0	43.4
April	46.6	43.2	44.9	29.84	3.0	1	10	3	8	1	2	1	4	16	14	...	1	3	16	6	51.9	37.2	44.5
May	52.4	49.6	51.	29.92	3.1	7	5	...	10	1	..	4	4	19	12	...	2	...	19	8	57.	45.6	51.3[*]

[*] The Register Thermometer was only used for the last seven months, and the mean of these was 42°.4; while that of the common Thermometer taken at 9 A.M. and 9 P.M. for the same period was 42°.3, being a difference of only 1-10th.

This table will show that from 1st December 1845 to 1st December 1846, or first 12 months, the average temperature was 50°.5, the quantity of rain 49.3 inches, and average height of barometer 29.86. The year 1846 was one, however, in which the mean temperature and amount of rain were greatly above the average—an occurrence which was universally noticed throughout the kingdom. The early part of 1847 was however quite the reverse, in which may be included December or the last month of 1846.

If, therefore, we take six months of the wet and warm year and add it to six of the dry and cold, an average pretty near the truth may be obtained. That a much larger proportion of rain than usual fell during the first six months of the period referred to, viz. from 1st December 1845 to 1st June 1846, will appear when it is stated that the quantity was 21.5 inches, while during the corresponding period of 1846–47, it was only 12.8, being a difference of nearly 9 inches for six months. The average at Rothsay for the period is 17.58, the annual amount being 38·5 inches; at Castle Toward 19.5, the annual average being about 44 inches. The average temperature may also be deduced more truly by taking the same period; for the mean heat of the summer and autumn of 1846 was extreme, while during the following winter and spring, cold and frosty weather prevailed in a similar ratio; there being 61 days in which the minimum of the thermometer was 32° or under, and a considerable number more, in which it was only one or two degrees above the freezing point. On the other hand, during the winter of 1845–46, there were not more than ten or twelve days of frost, and these of an extremely mild character. For the sake of convenient reference we have thrown the meteorological results of this year, with respect to temperature and rain, into a tabular form.

TABLE II.

Showing the mean temperature and quantity of rain for the year 1846-47.

Month.	Therm.	Rain.	Month.	Therm.	Rain.
1846			1847		
June	62.2	4.3	January	38.1	3.0
July	58.9	6.2	February	37.9	1.3
August	60.9	4.2	March	42.8	1.2
September	57.9	3.0	April	44.9	3.0
October	50.3	5.1	May	51.0	3.1
November	44.9	5.0			
December	37.1	1.2	Mean	48°.9	40.6 inch.

Highest temperature 80°, June 18th 1846, lowest 20°.5, February 9th 1847.

Highest barometer, 30.75, March 3d 1847; lowest, 28.6, 19th November 1845, with a tremendous gale of south wind and very high tide.

Wind east 61 days, west 41, north 37, south 88, north-east 17, north-west 42, south-east 43, south-west 36.

Fair days 212, rainy 153, including 8 of snow and hail, thunder 8, gales 5, breezy 162, including windy days, cloudy 99, including 21 hazy and foggy days.

The temperature of Largs, according to this table, is 48°.9, but deducting the fractions, we may assume that in a series of years the average would be about 48°.

In the New Statistical Account of Scotland, under the head of the parish of Largs, it is stated that "the quantity of rain which falls at Largs has never been ascertained, but there is no doubt that it is considerable." It appears, however, that experiments have been made on this subject many years ago. Mr Wilson's Account of Renfrewshire is stated to contain the following table of the quantities of rain in the places mentioned :—

Years.	Dalkeith.	Bothwell Castle.	Glasgow.	Largs.
1809	28.552	24.440	25.132	36.624
1810	25.636	25.010	21.433	38.714*

This gives a mean of nearly 39 inches of rain annually for Largs. According to Table 2, it is 40.6 inches.

The greatest amount of rain accompanies a south wind, and when the weather is changeable, often in a short period after the wind has veered to this direction; but if the weather has been fine for some time, a day or two may elapse before this occurs, and then it is generally copious and continuous, and frequently accompanied with a gale and fall of barometer. A south-east wind is also often accompanied with much rain, and occasionally the east carries with it a copious flood, although the barometer does not always simultaneously descend in this latter case. From the south-west the rain generally comes in comparatively short but copious torrents, accompanied with wind; the western breeze brings lighter showers, and a northern is generally cold and dry. The temperature is almost uniformly elevated when the wind changes to the south or south-west; while, on the other hand, a change to the north or east causes a fall in the thermometer. The feelings of cold produced by an east wind are however greater than what is often indicated by the thermometer, which may possibly arise from its power in rapidly abstracting heat. South and west winds generally prevail in summer and autumn, north and east in winter and spring. In the year 1846–47, during 165 days, southern and westerly winds prevailed, exclusive of those from the south-east and north-west, which amounted to 85. During fair weather, with a south or west wind, the air at Largs has a light, bracing, exhilarating effect, and although it contains a considerable amount of dissolved vapour, yet generally on these occasions, its capacity for more is so great, that evaporation goes on with great rapidity, and hence the feeling of dryness and elasticity. This is no doubt greatly promoted by the porosity of the soil. The alternations from a drenching rain in the morning, causing a rapid flood in the rivers and pools of greater or less magnitude on the roads, to a beautiful and shining day, which obliterates, in a few hours, all traces of the excess, are often very remarkable. It is thus that, in a climate where a considerable amount of rain falls, the atmosphere maintains its light and elastic character, and in calm weather this is much promoted by the gentle breeze which very generally blows from sea to land during the day, producing in summer a cool and refreshing effect; in the evening, however, it is sometimes reversed, by taking an easterly direction. This occurrence is generally explained upon the supposition that the colder air over the ocean is attracted to the warm region on the land during the day; while during the night, the air on the land becoming the colder, blows down from the mountains to the warmer valley below. In corroboration of the generally dry state of the air, we may mention that a few experiments connected with this subject were made with Daniell's hygrometer, not such, however, as from their extent or accuracy to be much relied on. In all the trials which were made during the winter of 1846–47, they showed that the difference between the dew point and the temperature of the air was generally at least 4 or 5 degrees, and in many cases more; thus indicating that the air was not saturated with moisture, and that, with a breeze, its evaporating power would be considerable. Thunder is considered rare in this district, and severe thunder storms have seldom occurred. About the beginning of August 1846, a severe thunder storm traversed over many parts of the country, but at Largs it was comparatively light. If this general opinion be correct, this partial exemption has not been accounted for. Gales of wind are not uncommon during autumn, winter, and spring, and are sometimes violent. In the month of December 1845, there were a considerable number. The weather was extremely mild, the thermometer being only in one or two cases at the freezing point, and the average temperature for the month being 40°.8; while in December 1846, there was only one from the north-east, the average temperature being 37°.1. These gales are generally from the south, west, and north-west, and in the latter case the sea rolls in with much violence and velocity from the Kyles of Bute, upon the harbour and adjacent parts of the town. In southern gales, Arran, Port Crawford, and the Cumbrays act as breakwaters to a certain extent, and render them rather less formidable than in some other watering places more to the south and less sheltered. The tides are generally much influenced by a high wind blowing from the ocean, and those which are extreme often accompanying the south; but when a spring-tide and a north-west gale occur simultaneously, the swell is sometimes at its maximum, but inundations do not now occur.

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* New Statistical Account of Scotland, Vol. vii. Parish of Lochwinnoch.

Soil-Water.—The old red sandstone is abundant on the shore, intersected in a few places with trap dikes. Upon this is deposited sand, gravel, or disintegrated sandstone; the soil is consequently very dry and porous. Water is readily procured by sinking to a moderate depth, and it is generally excellent, very rarely having any other mineral impregnation than what is common in well-water, the influx of sea water from a high tide rarely occurring. For drinking and all culinary purposes it is well adapted, but river or rain water being somewhat softer is sometimes employed for washing.

Population.—Diseases.—The population of the town may be estimated at upwards of 3000. The strangers who visit Largs in summer are computed to vary from 500 to 1000. The number of respectable families who reside permanently in the town and neighbourhood is very considerable, with the usual proportion of shopkeepers, artisans, labourers, and a considerable number of weavers. No peculiar diseases are indigenous among the natives, and in general they are a healthy and long-lived race. Dyspeptic affections are more frequently met with than might naturally be expected in such a healthy locality. Those, however, often occur in persons of sedentary habits or from errors in diet. Epidemic and infectious diseases, such as scarlet fever, simple and typhus fevers, measles, hooping-cough, &c. occur here as among similar communities of Scotland. During the summer and autumn of 1846, gastric and typhus fever prevailed to a considerable extent in the parish, chiefly the former, but in general it was mild, the mortality being 14 or 15 out of probably 200 cases. Some of the most severe cases occurred in children or young persons; the cerebral symptoms sometimes resembling idiopathic affections of the brain. My friend, the Rev. John Kinross, minister of the parish, has informed me that the mortality in the parish during the year 1845 was 67 or 68, and during 1846, 99, including strangers. The population was 4058 in 1841, but it must have increased to the amount of several hundreds since that period. In 1790, the Rev. Gilbert Lang estimated the population above eight years of age (examinable persons) about 805, and the deaths between 20 and 30 annually; but if small-pox or epidemic fever prevailed, above 40.

Fairley.—This is a village nearly three miles south of Largs, on the shore, and containing about 400 inhabitants. It is situated at the base of an elevated platform of ground, and is thus to a certain extent protected from the east wind; although during a gale it sometimes eddies down upon the houses. It is exposed somewhat to the west and south-west winds, but is upon the whole remarkably well sheltered, and is a quiet and beautiful spot, probably a little too close in the centre of the village during hot weather. The soil along the shore is dry, sandy, or gravelly.

ARRAN.

This island is situated in the Frith of Clyde, south of Bute, and is about 20 miles long by 12 broad. The southern part of the island is comparatively low and cultivated, while the northern portions present probably the most grand and picturesque scenery to be met with in this country. The watering-places chiefly resorted to are Brodick and Lamlash, both in the parish of Kilbride, which occupies almost the whole of the eastern part of the island, being about 20 miles in length. Brodick is situated in a bay regularly curved and about two miles in length, having a beautiful beach of fine sand and a fine level plain, on which are situated many cottages and villas. The situation is extremely beautiful, and the view of the opposite coast extensive. Lamlash is about six miles south from Brodick, is also situated in a deep and most extensive bay, about three miles in length, and although not so picturesque as Brodick, is still very beautiful. The Holy Isle stands in the mouth of the bay, a small conical and singular hill nearly 900 feet high. On each side of this island there is a fine entrance to the bay for all kinds of vessels, where there is a most secure harbour. The climate of these places is mild, but rather humid, there being little intense heat or severe cold, although the elevated mountains in the vicinity, such as Goatfell, are generally in winter covered with snow. Snow rarely lies more than a day on the lower grounds and valleys near the shore. The Rev. Dr M'Naughton states, "it must be acknowledged, however, that what the parish wants in snow is abundantly made up to it in rain, of which few places, even in the Hebrides, receive a more liberal share." According to the same authority, the prevailing winds are the south and west, which generally bring rain; and he is of opinion that the amount of rain which falls in different parts of the parish varies considerably, the places shut in among the mountains having the amplest share. "At the Whitehouse, in the neighbourhood of Lamlash, the quantity that fell in 1833 was 66 inches and 6–10ths; in 1834 57.6, and in 1835, 73.7." This would give an average for these three years of 65 inches of rain annually. Fogs and harrs are unfrequent, as well as days of constant rain; mornings of drenching flood being often succeeded by bright and beautiful days. Notwithstanding its moisture, it is a very healthy island, no peculiar diseases prevailing there, and it contains many aged people. Both Brodick and Lamlash are extremely well sheltered; the latter, it may be almost literally said, is defended on all sides, although squalls are said sometimes to occur in the bay, probably from the confliction of opposite currents of winds entering at each side of the Holy Isle. The soil on the shore is generally dry, being sandy or gravelly; but the accommodation in regard to houses is not so good as might be expected in such delightful retreats. In the account of the parish of Kilmorie there is quoted a table of the weather for 1836, furnished by Robert Stevenson, Esq. civil engineer. It is taken from the journal kept at the lighthouse of Pladda, which is a small island situated at the southern extremity of Arran. The mean temperature of that year, as ascertained by observations taken at 9 A. M. and 9 P. M., is 47° 6′, and the fall of rain 53.65 inches. West is the prevailing wind.

MILLPORT.

Millport is situated in the larger Cumbray, a small island in the Frith of Clyde, between two and three miles west from the parish of Largs. The town and landward part of the parish, which is entirely agricultural, contains nearly 1000 inhabitants, which number is sometimes doubled by the addition of summer visitors. It is situated in a bay, has a southern exposure, being somewhat open to the sweep of gales from the Atlantic Ocean, but is otherwise well protected from the north and east; particularly Kames Bay. The soil along the shore is sandy, but in some places the ground is rocky. The climate is mild, the range of heat and cold being small; and we should think also moist, although the Rev. James Drummond is of opinion that "the adjacent hills, both in Bute and in the mainland, being higher than those in Cumbray, more naturally attract the clouds; hence in Cumbray there is less moisture than in either of these places, and consequently a more genial atmosphere." The prevalent winds are the south and west, both of which frequently bring rain. Snow rarely lies long on the ground, and severe frosts are very unfrequent. The native inhabitants are strong and healthy, and no particular disease prevails among them. Millport is a very pleasant, rural, and retired watering-place, has beautiful views and a sheltered walk through the centre of the island. From its mild climate, dry soil, and good accommodation, it has within these few years become a favourite resort.

ARDROSSAN.

Ardrossan is situated in Ayrshire, in a parish of the same name, which is bounded on the south-west by the Frith of Clyde for three or four miles.

The town is recently built, is provided with excellent accommodation and an elegant bathing establishment, but is somewhat exposed to southern or south-western gales. The soil on the shore is dry and sandy, and there are excellent walks along the beach. The harbour of Ardrossan is built on an extensive scale, and is well protected. The climate is mild and healthy, there being little intense heat or excessive cold, and there is very generally a sea breeze. We are not aware of any particular register of the weather that has been kept at Ardrossan; but the Rev. David Landsborough of Stevenston, an adjoining parish, has made a series of meteorological observations at the manse, situated about a mile from the sea, and between sixty and seventy feet above its level. He has observed the thermometer only once as high as 84° for seven years, and only once as low as 18°. The average height of the thermometer, taken at ten o'clock A. M. for 1835, was 50°.88′, and for 1836, 48°.93′; the mean of which is 49°.9′. The prevalent wind is in general the south-west. The climate of this parish is mild, and a considerable amount of rain falls, but the sandy soil absorbs it rapidly, which, united with the sea breeze, soon gives the air its usual dryness and elasticity. The finer kinds of fruits, such as figs, vines, peaches, &c. ripen in the open air; delicate plants thrive well, and myrtles stand the winter without any cover. Longevity is also frequently met with in these two parishes.

Saltcoats, a considerable town on the coast, partly in the parish of Ardrossan, and containing four or five thousand inhabitants, is also resorted to as a watering place, but is now not so frequently visited for this purpose as at a former period. Some general and shipping trade is carried on in it.

ROTHSAY.

Bute is an island in the Frith of Clyde, about eighteen miles in length, and five or six in breadth. The parish of Rothsay comprehends the whole of the northern part of the island, and the town is situated at the head of a bay on the north-east side. This bay is remarkably well sheltered, perhaps unsurpassed in this respect by any other in any watering place on the south-western coast. What has been denominated the west is, however, less sheltered from east winds than the east bay.

The valley in which the old part of the town is situated is exceedingly well protected from injurious winds.

The population is nearly 6000, and the inhabitants consist of persons engaged in the usual occupations of country towns, many lodging-house keepers, with a certain proportion engaged in cotton factories. There is good accommodation of all kinds, and the houses are numerous, and extend along the shore to the extent of several miles.

The climate of this district, from its insular character and excellent shelter, is very mild and soft, although somewhat humid; indeed, in summer, it is sometimes close and oppressive, from a deficiency of sea breeze. Snow lies for a very short period on the island, and frost is rarely severe. There are no peculiar diseases; fever and our ordinary epidemics sometimes prevail, as they now do in almost every town where there is a working population.

A very particular register of the weather has been kept by Mr Thom of Ascog, and a very interesting table, drawn up by him, is quoted by Sir James Clark, from which it appears that the mean temperature of Rothsay is 48°.25′, and the mean quantity of rain annually 38.5 inches. There is, however, an extended table, quoted by the Rev. Robert Craig in the New Statistical Account of Scotland, in which the results differ, to a certain extent, from those just mentioned. The amount of rain, as measured by rain-guages at Mountstuart and Rothsay, and as calculated from the year 1821 to 1839 is 47, and when deduced from the averages given of each month from 1800 to 1839 is 48.32. The smallest amount of rain fell in 1803, being 38.45 inches, and the greatest in 1811, being 71.37. In another table, extending from 1828 to 1839, the average is 48.19 inches, and the average temperature 47.18. The soil is of a gravelly or sandy character along the shore; the roads are dry and good, and several of the walks are well sheltered.

Port Bannatyne is a small village, situated in a bay two or three miles to the north-west of Rothsay, where there are also some tolerably good lodging-houses. It is a quiet rural place, somewhat close and humid, but otherwise very pleasant and retired.

DUNOON AND KILMUN.

Dunoon is situated in the district of Cowal, Argyleshire, and on the northern side of the Frith of Clyde. It extends for several miles along the shore, from near the head of the Holy Loch to near Toward Point. In some situations, there is a considerable space of level ground, in others the acclivity is greater, and the hills behind are of considerable elevation. The rock on the shore is of a slaty character, and there is a gravelly or sandy beach well adapted for bathing. The west bay has a fine sweep, and is well sheltered from east winds, but both east and west are very beautiful. The soil on the shore is dry, and consists of a sandy loam. The scenery in the neighbourhood is extremely fine, and among the Highland hills and lochs grand and magnificent, the sea view being also extensive. The greater part of the town is of very recent erection, but it is rapidly increasing, probably more so than any other watering-place on the Clyde. The accommodation is excellent, cottages and villas are numerous, and the roads good and dry. The population of the town during summer amounts to several thousands, the larger proportion of which are temporary visitors. The climate is mild, as indicated by the successful cultivation of delicate plants. Snow rarely remains long on the ground, except on the neighbouring hills. Rain is, however, frequent and copious. A very valuable register of the weather has been kept by the late Kirkman Finlay, Esq. of Castle Toward, which is quoted by the Rev. Dr Mackay in the New Statistical Account of Scotland. This situation is fully two miles from Dunoon along the shore, and has a south-western exposure. The mean of the thermometer, taken at nine A. M. for the year 1840, was 47°.39, for 1841, 46°.6, and for 1842, 48°.1, which gives an average temperature for these three years of 47°.3. The amount of rain for 1840 was 43.3; for 1841, 46.4; and for 1842, 39.37, being an average of 43 inches for these three years. But as the instruments were placed 120 feet above the level of the sea, and at the distance of half a mile to the north, the mean heat may be estimated at nearly half a degree higher, or nearly 48°. Taking the mean of the three years mentioned, there were 169 wet and stormy days, 186 fair, 32 frosty, and 9 in which there was snow or hail. The natives of the district are healthy and long lived, and subject to no peculiar diseases.

Kilmun, a small village on the opposite shore of the Holy Loch from Dunoon, is situated at the base of a pretty high range of hills, along which houses and villas are built to the extent of more than a mile. There is very little space generally of level ground on the shore, and this near proximity to the base of the mountains probably renders it a little more damp than Dunoon. It is, however, a well sheltered, quiet, and pleasant situation, and the accommodation for summer visitors has been considerably augmented of late years.

GOUROCK.

Gourock is situated in the parish of Innerkip, Renfrewshire, is about two miles west of Greenock, contains a considerable population, and extends along the shore between one and two miles. It is a favourite and thriving watering-place, and its proximity to Glasgow, Paisley, &c. renders a visit to it very easy and convenient at all hours during the day. The old part of the town on the east has been in existence for a good many years, is situated in a fine bay, and is well sheltered. From Kempoch Point, near the quay, westward, the houses are larger and more commodious, and the scenery more beautiful; but the situation is more exposed to gales from the west or north-west. East winds chiefly affect the houses in the vicinity of the harbour. On the south the town is defended by a range of hills. The soil is dry and gravelly, the rock on the shore being generally sandstone. Abundance of fresh water is sometimes with difficulty procured. The climate is mild, and somewhat moist, showers being frequent; but it is nevertheless a very healthy place, and is very advantageously situated for people in business who require a prolonged residence at the coast. As we are aware of no meteorological observations that have been made in this place, we shall refer to Greenock, which is only a short way distant, with, however, a more direct north-eastern exposure. The Rev. Dr Macfarlane states that the average amount of rain which fell in Greenock, as ascertained by Crichton's pluviometer, during the years 1836, 1837, and 1838, was nearly 35.34 inches. It is also stated that, during a winter of unusual severity, the thermometer did not fall lower than 24°. We are aware that a much greater annual amount of rain has been assigned to Greenock, and that some controversy has existed on this subject; but as the position of the rain guages with regard to altitude of situation or shelter may possibly account, to a greater or less extent, for the diversity of results, we shall not farther attempt to discuss the matter.

HELENSBURGH.

Helensburgh is situated in the parish of Row, Dumbartonshire, at the mouth of the Gareloch. It contains upwards of 2000 inhabitants, and consists of an extensive range of houses fronting the beach, with numerous cottages and villas behind and along the banks of the Gareloch. It is pretty well sheltered from wind by the hills to the north and east of the parish, but is to a certain extent open to the south and south-west. Its climate is mild and somewhat moist, showers being frequent, but the thermometer seldom descends in winter to a low point. In a table given by the Rev. John Laurie of Row, and transcribed from observations made by the late Duke of Argyle, while residing at Ardincaple, it would appear that the amount of rain and the mean temperature, as deduced from noting the maxima and minima of the thermometer, are as follow:—

1832.	1833.		1834.	
Therm.	Therm.	Rain.	Therm.	Rain.
49.361	47·919	42·14	50·49	42·58

The mean temperature, therefore, for these three years, will be 49°. 25′, and the average amount of rain for two years 42·36 inches.

Least height of thermometer 26° in January 29th; greatest height 83°. 5′, July 31st, during the year 1834. In the same year, there were 172 dry and 193 wet days, 133 overcast, 106 fine and very fine, and 54 stormy. There were of easterly winds 98 days, of south and south-west winds 91, west 132, north and north-west 43. Thunder storms are said to be rare. The soil is dry, gravelly near the shore, and the rock is chiefly sandstone, which rises to some height behind the town. This town has advanced pretty rapidly to its present size, has excellent accommodation of various kinds; the scenery in the neighbourhood is interesting, particularly about Row and all along the banks of the Gareloch, while its proximity to Glasgow renders it very convenient. The natives are healthy and long-lived, and no particular diseases prevail in the parish.

ROSENEATH.

Roseneath is situated in the county of Dumbarton, on the opposite side of the Gareloch from Row, and is a small village, with a few villas along the shore.

In form the parish is a peninsula, and is connected with the northern part of Dumbartonshire by a narrow isthmus. The village and almost the whole parish is remarkable for its beauty and attractive scenery, and in this respect is not surpassed by any other on the coast. The village is exceedingly well sheltered, and the climate is very mild, as indicated by the growth of delicate exotics in the open air. The temperature rarely descends very low during winter; and the prevalent winds are from the south and south-west.

The Rev. Robert Story states, that " the average rain is certainly not greater, but it is, without doubt, more frequent than in the neighbouring districts of Renfrewshire and Argyleshire." This, he thinks, is owing to the mass of vapour which, driving up the estuary of the Clyde, generally discharges " the tail of the shower" upon Roseneath, whether it diverges to the mountains of Cowal on the left, or to those of Renfrewshire on the right. This phenomenon we have noticed, and have no doubt, from the projecting position of the lower part of the parish, that it is correct. The soil in the upper districts is marshy, drier on the shore, and the air on the whole rather moist, but there are few fogs, and the natives are very healthy. Mr Story states that, " from a calculation, regarded as accurate, of all the children born to the heads of families now in the parish and resident for the last forty years, $\frac{85}{100}$ are still alive." Instances of great longevity are not unfrequent.

General Characteristics of the Watering Places on the Frith of Clyde.

In many respects the watering-places on the Frith of Clyde resemble one another, but particularly regarding temperature, amount of rain, prevalence of winds, soil, scenery, &c. In all the cases where the temperature has been ascertained with any degree of accuracy, there is a tolerably near approximation, and the general average is higher than that of inland towns not very far distant. Thus the mean temperature of Glasgow is 46°. 67′; being the mean of the years 1834, 1835, and 1836;* of Edinburgh 47°. 31′; the average mean temperature of Scotland 46°. 36′, according to Sir James Clark; while that of Rothsay is 48°. 25′; of Castle Toward, nearly 48°; of the Gareloch, about 49°; of Largs, 48°. 9′. We have also the concurrent testimony of all those who have been accurate observers of the weather in these localities, in corroboration of these statistics; for all agree in stating that the cold is not so intense, and the climate altogether milder than in the interior of the country. As far as we are enabled to judge from the data in our possession, and from observation, the mean temperature of the various watering-places on the Clyde is about 48°. There are, no doubt, variations among them; and those that are situated most to the south and are exposed most freely to the sweep of the Atlantic Ocean will have the higher mean temperature. Thus at Pladda, Arran, the mean temperature of the year 1836 was 47°. 6′. The temperature of that year, however, was generally in Scotland lower than the average. Thus at Rothsay it was 45°, the average of many years being 48°; and at Glasgow 44°. 5′, the average of three years being 46°. 6′.

The amount of rain is very considerable in all the places that have been noticed, and is greater than what occurs in the interior of Ayrshire, Renfrewshire, and Lanarkshire, except in very mountainous districts, and much more than on the eastern coast. At Pladda, 53·65 inches have been collected, and in the neighbourhood of Lamlash 66·6; at Largs, 38 to 40; at Rothsay, 38·5; at Castle Toward, 43; at Helensburgh, 42·36; at Greenock, 35·34; and at the Isle of Man, 37·65. The amount which falls in Arran appears to be much greater than in the other places on the Clyde, when the quantity has been measured; and although it is probable that, being situated in the mouth of the Atlantic, and the land in the island being so mountainous, more rain descends there than in situations more to the north; yet it is not easy to account for the whole difference on this supposition. May not the varied situation of many rain gauges sometimes account for diversities of quantities collected in similar situations? For example, a rain gauge placed on the ground and one on the top of a contiguous house will not collect the same quantities of rain; neither will one at the level of the sea and one on the sides of a mountain, five or six hundred feet above this level, be the same in their results. Thus at Greenock one pluviometer was placed on the roof of the infirmary and another in the garden behind. In 1836 the one showed 41·89 and the other 62·10; in 1837, the one 32·39 and the other 49·15. Also, at the garden of Castle Semple, Lochwinnoch, a mountainous and inland parish of Renfrewshire, adjoining the high lands of Largs, during the year 1830, the rain-gauge near the surface of the ground showed 55·51, and the other, at some elevation above it, 43·95 inches. There is therefore some uncertainty in comparing the results of different rain-gauges; but as most of these, for the sake of convenience, are not much elevated above the surface of the ground, this objection does not so frequently apply as the diversity of exposure. They may, however, be fairly enough employed as approximations in estimating the comparative quantities of rain that fall in different places. The average amount of rain for the six different watering places that have been quoted is 42·14; but allowing for variations, the mean may be estimated as about 40 inches annually for the district on the Frith of Clyde.

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* Dr Cowan's Vital Statistics of Glasgow.

The number of days on which rain occurs may also be noticed, although not particularly characteristic. Thus at Pladda there were of rainy days, 118; Bute, 157; Castle Toward, 169; Largs, 145; Gareloch, 193; Isle of Man, 155, which we include for the sake of comparison. In the districts above mentioned, the number of wet days appears considerable, with the exception of Pladda, but it must be remembered that in this case, though there were only 118 in which rain occurred, 215 were foggy. The year 1836 was, however, one in which the weather was very unfavourable. With regard to the Gareloch, showers sometimes descend there from the Highland hills, which do not extend to some of the other districts; at the same time we are inclined to think that the number referred to is the extreme limit, being the result of only one year's observation, viz. 1834. In some of the inland towns quoted by Sir James Clark, the number of rainy days is also very considerable. Thus London is 178; Kendal, 176; Bristol, 160; Alderly, Cheshire, 188; Penzance, 155 and 164; Undercliff, 144; Cove, 138. Rome is 117, and Madeira only 73. In drawing conclusions regarding the frequency of rain in different districts of this coast, it may be remarked generally, that those which are situated at the base of an elevated range of mountains, have more frequent showers than those which are more distant, and we have frequently observed rain formed in the recesses of the Highland hills, descend gradually and skirt along the shore, the great bulk being expended before it reached the opposite shore of the estuary. Thus Dunoon, Helensburgh, Gourock, and, to a certain extent, Rothsay, may have more frequent visitations of showery weather than some other situations more to the south. Showers are often developed on the high ground adjoining the Kyles of Bute, which descend partly on Bute, and afterwards skirt along the Dunoon hills and up the valley of the Clyde, but which only to a small amount reach the opposite shore of Largs. On the other hand, the quantity of rain, which is generated by the south winds, descends in greater abundance upon the more southern watering places, and thus may fully compensate, in point of amount, for the little difference which may exist in frequency of visitation. The prevalence of similar winds also constitutes a point of resemblance, and, for the sake of comparison, we shall take the southerly and westerly winds. At Castle Toward, Dunoon, there were 145; Largs, 165; Rothsay, 189; Pladda, 142; but in this case the whole number of days in the year does not seem to be included; Stevenston, 180; and Gareloch, 223. It may here be remarked, that the valley of the Clyde appears to have some influence in modifying the direction of the wind; and we have noticed that a due south wind is sometimes diverged to a small extent to the west, after passing the turning point at the Cloch Lighthouse. The range of temperature is also, in all the districts, much less than in inland situations, never descending so low in winter, or being so high in summer.

According to the observations of Mr Thom of Bute, the thermometer for twelve years never fell lower than 20° above zero; while on one of these occasions it indicated 5° below zero, at the Glasgow Botanic Gardens. In 1838, the thermometer at Helensburgh was 15° above zero, and at the Glasgow Botanic Gardens it fell to 1½° below zero. At Largs, during the years 1845–6 and 1846–7, the minimum was never under 20°, nor the maximum above 80°; at Gareloch, 26° is mentioned as the lowest and 83° the highest point; at Stevenston it has only once for seven years been observed so low as 18° or so high as 84°.

The mildness of the climate, as indicated by the successful cultivation of delicate and exotic plants, is specially noticed by almost all the contributors to the New Statistical Account of Scotland, as well as the unfrequency of snow storms and its speedy dissipation when snow does occur. The remarkable accordance in their allusion to the longevity of the natives may also be made to bear upon the question; for, in the absence of regular bills of mortality, this is probably the strongest proof of the healthiness of any particular climate. Another very important element in the character of this coast is the dry soil, and it is one which has an immense influence upon its sanatory qualities; for owing to this the great exuberance of moisture, which otherwise might be very prejudicial, is speedily absorbed; and in situations that are exposed to a moderate breeze, or which are not too close, the air is soft, elastic, and sufficiently dry. There is also some similarity in the character of the gales, being frequently from the south or southwest; and though they are often to a high degree boisterous, yet being in many cases accompanied with a comparatively high temperature, are not so disagreeable or injurious as those from a northerly or easterly direction.

In summing up the general qualities of the district on the Frith of Clyde, it may be characterised as having a mild and humid climate, the range of the thermometer being less than in inland situations; a preponderance of southerly and westerly winds, often accompanied in mild weather with a soft and refreshing breeze; beautiful sea, and magnificent mountain scenery; a dry gravelly or sandy soil; few invasions of snow storms, occasionally gales of tempestuous violence, accompanied with floods of rain; but above all, it may be truly designated a land of health and longevity to its natives. It resembles the south-western coast of England, such as Torquay, Sidmouth, &c. in its moist climate and equable temperature, but its mean heat is several degrees lower than that of these places.

The next important point for consideration is its influence on the general health, and in the treatment of diseases; and be as it would here be impoſsible to particularize minutely, and at the same time of no special practical utility, we shall notice the subject under three heads. 1. As a means of invigorating the general health. 2. In pectoral diseases. 3. In dyspeptic and chronic diseases.

1. The inhabitants of cities and large towns, such as Glasgow, Edinburgh, Paisley, &c. constitute the large proportion of those that frequent our Scotch watering places, many of them for recreation and rural enjoyment, others for some indefinite consciousness of debility or general indifference of health, but all with some sanatory view to themselves or families, rightly apprehending that it is needful occasionally to blow off the contaminated air of a large city by a western breeze. And here we would remark, that the advantages of a prolonged residence do not seem to be appreciated by a large class of summer visitors. One or two months at most appear to them amply sufficient for the purposes of health; while the tedium of a country life for five or six would be contemplated as something like a temporary banishment. This is undoubtedly an erroneous calculation; for although one or two months are almost always productive of benefit, and sometimes to the requisite extent, yet, under such limitation, the large proportion of cases, especially delicate children, in a short time relapse into their former enfeebled state. The time is too short for the stamina to become permanently fixed in their improvement. Six months of pure air might possibly enable them to contend against six months of impure; but one or two cannot reasonably be expected to counteract the evil influences of ten or eleven. In almost all the watering places of the Clyde,

September and October are equally calculated to produce benefit as the warmer months of summer, and sometimes more so, for the requisite exercise can then be taken with much less fatigue. Indeed, the neglect of due exercise, and sometimes of bathing, often account for the small amount of advantage derived from a residence at the coast.

2. In pectoral affections of a consumptive kind, a mild and equable temperature, with some degree of softness in the air and a sheltered residence, are in general best adapted for their treatment. In this respect there are several places on the Clyde suitable for a summer residence, such as Rothsay, Row, Roseneath, Arran, &c. ; but their mean temperature being lower than the south-west of England, the latter is preferable during winter. At the same time, circumstances of various kinds are frequently opposed to a long journey, and in such cases one or other of these places may be considered equal to any of a similar kind in Scotland. Again, in estimating the value of the different situations for a winter residence, the following additional points ought to be taken into consideration, viz. the distance of the dwelling from the shore, the ground for exercise, and the accommodation ; for a consumptive person ought to reside at some distance from the shore, so as not to inhale constantly the humid and often salinely impregnated air from the water ; which appears to be more potent in this respect in the more southerly watering places ; the ground for exercise ought also to be sheltered, particularly from northerly, easterly, or boisterous winds of any description ; and the lodgings ought to be dry, and carefully secured from currents of air. In all these respects we think that Rothsay has some advantages, a reputation which it has long enjoyed ; and probably the valley in which the old part of the town is situated, being at a little distance from the shore, is in many respects the most eligible locality, although the east bay is well protected from east winds. The villages of Row and Roseneath, as well as Arran, may also be found suitable. In bronchitic affections, such as chronic inflammation of the trachea and bronchial tubes, the same rules apply as in consumptive affections ; but if the patient be of a relaxed constitution, with copious expectoration and atonic derangement of the digestive organs, some of the other situations which have a more free and bracing atmosphere may be selected.

3. For the cure of the multifarious tribe of digestive derangements the sea coast is often resorted to, and generally with some benefit, if the patient has previously been a resident in a large town. He inhales purer air, his exercise is generally more regular and extended, his mode of living is often more moderate, and the action of the skin is stimulated by bathing ; it is, therefore, not wonderful that improvement should follow. At the same time it may be remarked, that derangements of the digestive

organs are by no means uncommon among the natives and permanent residents at the coast, which may partly be accounted for from their mode of living; but the humidity of the atmosphere in many places is also a cause more or less powerful. If the coast be selected, a situation, therefore, should be chosen where the air is more cool and bracing, such as Largs, Helensburgh, Dunoon, Ardrossan. In the ordinary form of dyspepsia, which often occurs in sallow, relaxed, and frequently spare individuals, accompanied with debility, languor, acidity, flatulency, constipation, &c. a dry inland and rather elevated situation has always appeared to us the most eligible for this modification of the disease. In almost all the irregularities of the bowels to which children in large towns are so frequently subject, and which are the fruitful sources of many corporeal evils, the coast is well adapted; and generally, if there be nothing organically diseased, and if the stay be sufficiently prolonged, the expected amendment is obtained. A sheltered and mild watering-place ought in general to be preferred for very delicate children; for, like tender exotics, they may be seriously injured, particularly before they become acclimated by exposure to an eastern or even a boisterous wind. If bathing with salt water be had recourse to, it ought occasionally to be intermitted, as it is sometimes liable to cause irritation of the skin, &c.—fresh water being substituted during the intervals. In general, however, this coast is not so well suited as some other situations of a drier and more bracing kind for patients affected with chronic diarrhœa, leucorrhœal affections, or uterine diseases, accompanied with copious secretions and relaxation of the system. In cachectic or broken-down states of the constitution, from intemperance or organic disease, it presents many advantages, and is generally productive of temporary benefit. During warm weather, the pure air and refreshing breeze from the south and west cool and invigorate the patient, and during winter he is not exposed to the same intensity of cold. In summer some of the more bracing watering-places ought to be preferred, but during the cold season he should select a sheltered locality, with all the appliances of comfortable accommodation.

Greenock, 1847.

ART. III.—*Notes of the Cases of several Men who were Wounded in the different Engagements on the Sutlege and its Vicinity, with Remarks.* By GEORGE WILLIAMSON, M. D., Staff-Surgeon, Second Class.

I.—WOUNDS OF THE HEAD.

No. I. Private William Garbett, 29th Regiment, on the 10th February, at Sobraon, received on the left side of the head, above the ear, a sabre wound, which appears to have cut right through the parietal bone. He was insensible for a month, and could not speak for some time after. The bone is now depressed, the skin adherent, and very tender to the touch. He has frequent giddiness, partial deafness, and some impediment in his speech in consequence. He was wounded by a musket shot in the right thigh, immediately above the knee-joint; the ball passed right through the muscular part of the limb.

No. 2. Private John Willis, 29th Regiment, at Sobraon, 10th February, received a musket shot, which passed through the right tarsus. When down, he was struck on the upper part of the coronal suture by another ball, which remains in the part, and has several times been felt, but could not be taken out. Several pieces of bone came away, and the wound healed two months after. He complains of pain in the head, giddiness when stooping, and confused vision. The wound broke out a second time on board ship, but it is at present healed. The foot is contracted, and he has lost in some degree the power of moving it.

No. 3. Private William Cathy, 31st Regiment, at Ferozeshah, when charging, a shell burst, and shattered the left wrist. The fore-arm was amputated by the circular operation twelve days after. At the same time another portion of shell struck him immediately behind the left scapula, where there are now two cicatrices. There is no evidence of its having penetrated the thoracic parietes. There is also a circular depression in the occipital bone from the same cause. This is painful to the touch, and he complains of headach and giddiness, with impaired vision.

No. 4. Private Patrick Dunn, 31st Regiment, at Moodkee, received a sabre wound, which amputated the middle and ring fingers, and the two distal phalanges of the forefinger of right hand. He was struck down by a sabre wound on the left side of the head, which fractured the bone, cut the ear and part of the cheek. The bone is now depressed, and he complains of giddiness and pain of the head.

No. 5. Private James M'Donnell, 9th Regiment, on the 21st December, at Ferozeshah, was struck in the left eye by a grape shot, which entirely destroyed this organ. The ball coursed round the neck, and was taken out below the occipital bone. He has now total loss of sensation in the left cheek, probably on account of the superior maxillary nerve having been wounded. The eyelids are united at the inner canthus. He remained on the field all night, and on the following morning, when lying on the ground, the enemy having returned to despatch the wounded, he then received a musket shot in the left shoulder-joint. The ball entered posteriorly, and passed out in front about two inches below the head of the bone. On the third day he was taken from the field. He has now very little power over the arm, and cannot move the shoulder-joint, which is partially ankylosed. Pieces of bone came out of the orbit fifteen days after.

No. 6. Private Andrew Smith, 80th Regiment, at Moodkee. A musket ball went in through the bridge of the nose on the right side, destroyed the left eye, and passed out over the left malar bone. Many pieces of bone have come away. The eyelids are much separated, and there are two small openings on the left side leading into the nose.

II.—WOUNDS OF THE THORAX.

No. 7. Private Martin May, 31st Regiment, was wounded in the left breast at Sobraon, by a musket ball, which entered the chest in the second intercostal space, about 2½ inches from the sternum, or at the junction of the cartilages with the ribs. He coughed up large mouthfuls of blood at the time of the accident, and in smaller quantities for a fortnight after. The ball still remains in the part. He was taken to the field hospital immediately, and on the third day was carried to Ferozepore; the wound was probed, but the ball could not be felt. Four months after the receipt of the injury, the wound healed.

On examining the chest, the left side is found to be of the same size as the right, and both expand equally on taking a full inspiration. Percussion is clear on both sides; and on applying the stethoscope, the respiratory murmur is heard distinctly over the whole chest, and not at all diminished around the cicatrix.

The man has a slight cough, with mucous expectoration; he has no pain in the chest except when coughing, and then he feels it in the breast extending from the wound to the axilla. He had dysentery in India in 1840; the abdomen is at present tumid, bowels regular, and he states that he is now in good health.

No. 8. Private William Haverty, 31st Regiment, when advancing to the batteries at Sobraon, was wounded by a musket ball in the right breast, which entered close to the sternum in the second intercostal space, passed obliquely through the chest, and was cut out from beneath the skin a few hours after, at the posterior aspect of the arm close to the axilla. At the time of the accident, and for twelve hours after, he was nearly choked with blood from the lung, which he continued to spit up in small quantities for the space of three weeks. There was at first considerable hemorrhage from the anterior wound, which, at the end of three months, seemed healed, but broke out afresh about a fortnight after, and finally healed at the end of three weeks. When the wound opened he expectorated some blood. The right side of the chest, in front and immediately below the clavicle, is a little flattened, and not so full as that of the left, which seems to be owing to the smaller size, and matting of the pectoral muscles. On running the finger along the second rib close to the cicatrix, the bone is felt to be slightly enlarged from ossific deposit—the ball having probably struck it and then entered the chest. On taking a full inspiration, the right side does not expand so fully and freely as the left, perhaps on account of universal adhesion of the pleura. Percussion elicits a clear sound over the whole of the thorax. The respiratory murmur is very distinct on both sides, rather more so in the right than the left. Close to the cicatrix, where the intercostal muscles have been destroyed, respiration is more clearly heard than at any other part; and on expanding the chest, a portion of the lung is, as it were, pushed between the ruptured intercostals. The thinner layer of muscles on the wounded side, compared to the other, and particularly at the wound, may account for the respiration being heard more clearly than on the sound side.

He states that he has slight cough, expectoration, and occasionally pain in the chest at the site of the wound. He is a stout muscular man, and apparently in good health.

No. 9. Sergeant John Leary, 50th Regiment. A musket ball passed through the right lung between the ribs, without fracturing any bone; it entered immediately above and close to the right nipple in the fourth intercostal spaces, passed through the lung, and made its exit opposite to the inferior angle of the scapula, and dropped down under his clothes. He got upon a gun carriage and was taken almost immediately to the field hospital; was sent to his own hospital shortly after, and remained there for three days before anything was done for him, partly, he states, on account of the scarcity of medical officers, and partly from his case being considered hopeless. He lost a large quantity of blood from the anterior wound, and expectorated large mouthfuls of blood immediately after the injury and for six days subsequently. On the third day portions of his jacket, shirt, and belt were taken out from the anterior wound, which was healed in six, and the posterior one in seven weeks. He has pain in the chest whenever he exerts himself, or when he coughs, but at other times he has no uneasiness.

He was wounded in the left leg and right fore-arm, the latter of which required to be amputated about its centre.

No. 10. Private Samuel Eldridge, 62d Regiment, at Ferozeshah, was struck with a grape shot in the left arm. The ball entered on the outer side of the head of the *humerus* (he was standing at the time with his side to the enemy), and appears to have gone backwards, entered the chest, and was taken out seven months after, at the anterior margin of the scapula, a little above its inferior angle; thirteen days after the accident he spat up large quantities of blood, and continued to do so for the space of three months. A portion of jacket and a piece of bone were taken out five months after, from an incision made at the posterior margin of the axilla. Several pieces of bone came away from the wound on the head of the humerus, and portions of the scapula from the posterior one. The shoulder-joint is now ankylosed, and the arm is wasted.

No. 11. Corporal Patrick Brennan, 29th Regiment, was struck, at Ferozeshah, by a grape shot on the inner side of the right *humerus*, opposite to the lower margin of the axilla. The course of the ball seems to have been changed by the bone, and directed into the chest. It came out behind and a little below the inferior angle of the scapula. Next morning he spat large mouthfuls of blood, and has done so on several occasions since. A portion of the bone came away from the *humerus*, and several portions of the ribs and pieces of his jacket have been removed from the wound on the posterior aspect of the chest. The arm is wasted, and its power and motion are impaired. He also complains of occasional shortness in breathing.

III.—WOUND OF THE DIAPHRAGM.

(*Hernia of Stomach and Colon.*)

No. 12. Private Thomas Fletcher, aged 40, an Irishman, a weaver, height 5 feet 6$\frac{1}{2}$ inches, stout and healthy frame; total service 20$\frac{6}{12}$ years, of which 19$\frac{6}{12}$ years were in India, where he had two or three attacks of intermittent fever.

He was wounded at Sobraon by a musket ball in the left side of the thorax, entering between the eighth and ninth ribs, and about four inches distant from their cartilages, and making its exit close to the transverse process of the eleventh dorsal vertebra, and between the eleventh and twelfth ribs.

He landed in England on the 13th January 1847, in good general health. He was admitted into Fort Pitt General Hospital on the 20th January, under observation for the wound in his side; discharged well on the 2d February. On the 11th February, immediately after taking dinner, he was attacked with vomiting and pain in the left side, over the spleen. This continued until the evening of the 12th, when he was admitted into this hospital. He could not account for his sickness; he had been quite well in the morning, and his bowels had acted regularly. On admission the skin was cold; the pulse 90, quick, small, and wiry; respiration natural; and pain was felt over the spleen, increased on pressure. On the 13th, the pain had left his side and shifted to the shoulder and clavicle; it was not acute. He was free from all other pain and all inflammatory symptom to the hour of his death; neither was there any pain on pressure in the re-

region of the stomach, nor any tension of the belly; but, on the contrary, an extraordinary hollowness, or drawing in about the *umbilicus*, resembling very much in appearance a man suffering from Asiatic cholera. His bowels could not be acted on, and the irritability of stomach and vomiting of all things swallowed continued to the last. He began to sink on the afternoon of the 16th, gradually became weaker, and died on the 18th February; it being eight days from the time he was first attacked with vomiting, and twelve months from the time he was wounded. The cause of the irritability of the stomach and the torpidity of the bowels was as obscure as on admission. The treatment consisted at first of an emetic, with a blister over the spleen. Afterwards full doses of calomel and opium, morphia and hydrocyanic acid frequently repeated; castor oil combined with croton oil, both by the mouth and injections; turpentine injections and fomentations to the abdomen; hot bath and hot bottles to his feet; beef tea injections —were the remedies administered.

INSPECTION OF THE BODY THIRTY HOURS AFTER DEATH.—*External Appearances.*—Body stout and well formed; skin shrivelled, more particularly that of the hands and feet; features contracted, and indicative of a person having died under great suffering; muscles rigid.

Cranium.—About 1 oz. of fluid at the base of the brain; the veins of the *pia mater*, as also those of the *velum interpositum* and choroid plexus, were congested; section of brain presented a number of bloody points. Weight of brain 3 lbs.

On opening the thorax, the greater part of the stomach, the transverse arch of the colon and omentum, were found in the left pleural cavity. The left lung adhered very firmly and closely to the walls of the chest, as low as the ninth rib, by adhesions of long standing; the lung was pushed to the upper part of the cavity, but on account of the adhesion to the ribs, it was compressed into a thin layer, which lined the walls of the thorax. The heart was also displaced, and lay behind and a little to the right of the sternum. The large curvature of the stomach, partly covered by omentum, lay in front, and first showed itself on opening the chest; the transverse arch of the colon was to the left of the stomach, and between it and the ribs; the stomach reached a little higher in the chest than the gut. In the diaphragm was an opening with rounded margin, 2½ inches in diameter, situated 2 inches to the left of the *œsophagus*. The peritoneum lining the diaphragm proceeded through the aperture, and was continuous with the *pleura;* the serous surface around this opening, although smooth and uninterrupted, yet presented some thickening, and an appearance of old cicatrization. In the pleural sac, close to the opening in the diaphragm, on its posterior and external margin, the stomach, colon, and omentum adhered firmly to the pleura covering the diaphragm and ribs, to the extent of a few inches; there were also two broad, thin, and loose bands of adhesion, about 8 inches in length, stretching from the omentum to the base of the lung and side of the pericardium. The stomach and colon were, however, loose and free in the pleural sac; the parts in the aperture of the diaphragm were free from adhesion, and not constricted; and the fingers were easily introduced through it, from the abdomen into the thorax.

The œsophagus, after penetrating the diaphragm in the usual place, took a sharp turn to the left side; the stomach then entered the thorax; a portion of the cardiac extremity of which still, however, lay in the abdomen, in front and to the right of the spleen; the larger part having entered, the chest curved round and descended to the opening in the diaphragm; the pyloric orifice lay immediately in the aperture. About a foot and a half of the transverse arch of the colon, with the omentum attached, were also in that cavity.

The structure of both lungs was healthy; the posterior aspect of the right was attached to the chest by adhesions of long standing.

The heart was of its natural size, and its structure healthy.

On tracing the small intestines five intussusceptions were found, the first situated at a foot and a half from the duodenum, and the other four were generally from eight to ten inches apart. The portions intussuscepted were in each about two inches. No vascularity or congestion was observed in the intussusceptions, or in any of the abdominal viscera. Both portions of the stomach, viz. that part in the chest and that in the abdomen, contained a quantity of dark fluid, mixed with portions of food and medicine. The *duodenum* and upper part of the *jejunum*, as far as the first intussusception, were distended with flatus. The small intestines below the invagination, although not distended, but in their normal condition, were coated with mucus, tinged with bile.

The *caput cæcum*, and ascending colon to where it entered the chest, were distended, and contained a quantity of fluid and hardened fæces. The transverse arch of the colon was distended with flatus and some feculent matter. The descending colon and rectum were empty.

The liver healthy; weight, 3 lbs. 4 oz. Spleen healthy; kidneys healthy; weight of right, 6 oz. 3 drs.; left, 6 oz. 2 drs.

On the skin, exactly opposite and corresponding to the opening in the diaphragm, were two old cicatrices on the left side of the chest, one situated between the eighth and ninth ribs, three inches and a half from their cartilages, where the musket ball had entered; and the other where it had effected its exit between the eleventh and twelfth ribs, close to the transverse processes of the vertebræ.

The ball in its course through the thorax must have wounded the diaphragm, and thus given rise to the hernia of the stomach and colon.

The parts forming the hernia are preserved, and placed in the museum.

IV.—Wounds of the Abdomen.

No. 13. Private William Mehan, 29th Regiment, at Sobraon, 10th February, when storming the intrenchments, was struck by a large shot, which carried off his right leg, a little above the ankle. While trying to get to the rear, he received two wounds from the bursting of a shell, one in the right thigh, which went through the fleshy part, and another entered in the right inguinal region, opposite to the external abdominal ring. It is difficult to say whether the shot wounded the intestines or what course it took. He was insensible for thirteen days. His belly was much distended, and of a dark colour; portions of his trowsers were taken about two months after from the wound in the abdomen, but no feculent matter came from it. At present, when moving about on crutches, he occasionally feels a heavy dragging pain in the lower part of the belly.

The right leg was amputated immediately after the accident by the circular operation, close to the knee-joint, by the field surgeons. The stump was covered by skin only.

V.—Amputation (by the Flap method).

No. 14. Private George Hacken, 16th Lancers, at Alliwal, received a gun-shot wound; the ball passed through the right elbow-joint. Amputation was performed three hours after, by the flap operation, by Dr Currie. A very good stump.

No. 15. Private John Smith, 31st Regiment, at Ferozeshah, was struck by a grape shot on the inner side of the left arm above the elbow-joint, and which lacerated the brachial artery. Twelve days after the accident secondary hemorrhage ensued, and the arm was amputated by the flap operation, two inches below the head of the humerus, by Dr Stewart, assistant surgeon, 31st Regiment. A good stump.

No. 16. Corporal William Heasman, 80th Regiment, at Ferozeshah, received a musket ball in left fore-arm, which ran up to the elbow-joint, where it made its exit. Amputation was performed by the flap operation twenty-two days after the accident, on account of gangrene. A good stump.

No. 17. Private Benjamin Oddy, 80th regiment, at Ferozeshah. A cannon ball carried the right fore-arm almost completely off at the elbow-joint. Amputation was performed in the middle of the arm by the flap operation. A very good stump.

No. 19. Private William Manser, 31st Regiment, at Alliwal. A grape shot struck left fore-arm and fractured both bones. The fore-arm was amputated on the field, within an inch of the elbow-joint, by the flap operation. He has perfect motion of this joint, but it will be difficult, if not impossible, to fit any apparatus which will enable him to use the stump.

No. 19. Private Andrew M'Gowan, 31st Regiment, at Budwall. A musket shot fractured the bones immediately above the wrist;—the fore-arm was amputated shortly after, by the flap operation, about an inch below the elbow-joint. The stump is too short to allow of an apparatus being fitted to preserve the use of the elbow-joint.

No. 20. Private Patrick Burns, 80th Regiment, at Sobraon, received a musket shot in the left wrist. Three hours after, amputation was performed in the lower part of the fore-arm, by the flap operation. A very good stump.

No. 21. Private Henry Carroll, 31st Regiment, at Ferozeshah, when charging, a shot carried away the middle, ring, and little fingers. The heads of the metacarpal bones were removed. The thumb and fore-finger are very useful.

No 22. Private George Barritt, 53d Regiment, at Budwall, received in left knee a cannon shot, which shattered the femur. The army retreated, and while lying on the field, the Sikhs passed over him, and were murdering the wounded; and just as one of them was about to fire at him, a chief interposed, seeing that he was badly wounded, and had him carried to their hospital, where a black doctor put his leg up in Bamboo splints. He was afterwards sent to his own hospital, and the thigh was amputated by the flap operation on the fourth day by Dr Murray. A very good stump.

No. 23. Private George Waring, 53d Regiment, at Alliwal, was wounded on the inner side of the right knee by a cannon shot, by which the leg was carried completely off. Amputation in centre of the thigh by lateral flaps was performed immediately after the accident. A well-formed stump was the result.

No. 24. Private Timothy Sullivan, 29th Regiment, at Sobraon, was struck, on the 10th February, on the right instep by a musket ball, which passed through the left ankle. The left leg was taken off on board ship on account of *necrosis* of the *tibia*, by the flap operation. The skin in front seems to have been dissected up so as to form an anterior flap, and the margin of the bone is in consequence very near the surface, and very thinly covered.

No. 25. Private Michael Cummins, 31st Regiment, at Ferozeshah. A grape shot struck the dorsum of his right foot, and passed out in the sole. The metatarsal bones of the three outer toes were amputated by Dr Stewart. The great and second toes remain, and are of great assistance to him in walking.

VI. AMPUTATION (by the Circular method).

No. 26. Private John Pigot, 62d Regiment, at Sobraon. A shell struck the left elbow, and carried the arm almost completely away. Amputation was performed the same evening by the circular operation. Seven days after the accident, sloughing took place, and considerable hemorrhage followed, but no vessels required to be tied. The bone is covered by adherent skin only.

No. 27. Private William Daxby, 16th Lancers, had, at Aliwal, his horse shot under him, and, when down, received immediately above left elbow a sabre cut, which fractured the bone. Amputation was performed in the middle of the arm by the circular operation. The bone is covered only by skin, which is adherent.

No. 28. Private John Dyas, 10th Regiment, on the 10th February, at Sobraon, received, above the right elbow, a sabre wound, which fractured the humerus; also a sabre cut over the pectoral muscles of the same side, another on the right cheek, and a fourth on left fore-arm, which cut the flexor tendons. He can move the fingers, but cannot close them or grasp anything with the left hand. The right arm was amputated on the third day, by the circular operation, in the middle of the arm. A good stump was the result.

No. 29. Private David Connors, 31st Regiment, at Ferozeshah. A grape shot struck the left elbow, and fractured the bones. The arm was amputated about its centre by the circular operation. Four inches of the whole thickness of the extremity of the bone came away, necrosed, about six months after. The end of the bone now projects, and it is covered by adherent skin only. Received a musket shot in left hypochondriac region, which did not penetrate the walls of the abdomen. The ball was extracted.

No. 30. Private James Smith, 31st Regiment, at Moodkee, received a musket shot in right elbow-joint. The arm was amputated two days after, by the circular operation. The end of the bone is covered with skin only. A portion of shell went through the calf of the left leg.

No. 31. Private Henry Benning, 53d Regiment, at Sobraon, was struck by a cannon ball on the inner side of right knee, and the femur was fractured. The thigh was amputated three days after, by the circular operation. The skin is adherent to the bone.

No. 32. Private John Edmonds, 53d Regiment, was wounded at Sobraon by a cannon shot, which struck the inner side of the right knee, and carried the leg completely off. The thigh was amputated immediately after, by the circular operation. The skin is adherent to the bone.

No. 33. Private Henry Allison, 53d Regiment, at Sobraon.

A cannon ball struck the inner side of right knee, and carried the leg completely off. He lay in the field all day. Amputation was performed in the centre of the thigh by the circular operation. The skin is adherent to the bone.

No. 34. Private Henry Omrid, 80th Regiment, at Sobraon. A cannon shot struck the right knee-joint, and fractured the femur. Amputation was performed in the centre of the thigh, by the circular operation, four hours after the accident. A very good stump was formed.

No. 35. Private John Wallbank, 31st regiment, had his right thigh fractured immediately above the knee by a grape shot at Moodkee. Amputation was performed three days after by the circular operation; the bone is covered by skin only.

VI.—CONTRACTIONS AND ANKYLOSIS.

No. 36. Private Timothy Ekees, 80th Regiment, at Ferozeshah. A musket ball entered the head of right humerus, and still remains in the part. The motions of the joint are much impaired; he can move it backwards and forwards, but cannot raise the arm from the side.

No. 37. Private Thomas Edwards, 80th Regiment. A grape shot went through the head of the right humerus, and came out at the posterior border of the axilla; a piece of bone and portion of his jacket were taken out. The arm is emaciated, and its motion much impaired.

No. 38. Private Michael M'Kearney, 31st Regiment, at Moodkee. A grape shot entered behind the left acromion process, and passed out opposite to the coracoid process; his stock, which was torn from his neck, probably changed the direction of the ball, and was the means of saving his life. Some small pieces of bone have come away; the motions of the shoulder joint are impaired, but the arm is not emaciated. A musket ball went through the calf of the left leg.

No. 39. Private Charles Stodgell, 29th Regiment, at Sobraon, was struck in the right humerus, immediately in front of the insertion of the deltoid, with a grape shot, which fractured the bone; the ball was taken out on the field, from the outer side of the arm, three inches above the elbow-joint. Several small and one large piece of necrosed bone have been taken away. The arm is slightly wasted; he cannot move the shoulder and elbow joints of his own accord, although their motions are perfectly free, but they can be moved by the assistance of the other hand.

No. 40. Private William Porter, 29th Regiment, at Ferozeshah, 21st December. A grape shot passed through and fractured the humerus about four inches above the elbow joint; the wound healed about seven months after. The bone is now enlarged from ossific deposit; the muscles of the arm, but especially of the fore-arm, are wasted and powerless and he cannot move the arm without the assistance of the other. The motion of the elbow-joint is perfect. He received a superficial wound in the lumbar region from a grape shot.

No. 41. Private William M'Nelly, 80th Regiment, at Ferozeshah. A musket ball entered on the outside of the right elbow, passed through the joint, and made its exit over the internal condyle; many pieces of bone came away. The joint is now ankylosed and the arm is in a bent position.

No. 42. Corporal Angus Logan, 3d Light Dragoons, at Ferozeshah, received a musket ball in the left elbow-joint, which entered on the posterior and outer side of the arm, between the external condyle and the *olecranon*, passed through the joint, and was taken out about a month after on the inner side of the *olecranon*. Several pieces of clothing were taken from the wound where the ball entered. Incisions were made in different directions around the joint for the escape of matter; the wounds are only now healed; the joint is ankylosed; he can move the fingers; the arm is in a bent position.

No. 43. Private Henry Henson, 29th Regiment, at Ferozeshah. A musket-ball struck the front of the right elbow-joint, passed through, and made its exit above the *olecranon*. Several pieces of bone have come away. The joint is ankylosed, much enlarged, and there are several sinuses which are healing. He has no pain in the joint, but the arm remains in a bent position.

No. 44. Private Joseph Parker, 3d Light Dragoons, on the 10th February, at Sobraon, was wounded by a musket shot. The ball entered on the posterior part of the fore-arm, a few inches below the elbow-joint, passed through the joint, and made its exit a few inches above and on its inner side. Several pieces of bone were taken out about six days after. The joint is now ankylosed, and he has not much power either in the arm or fore-arm.

No. 45. Private James O'Brien, 3d Light Dragoons, received a grape shot in the left elbow-joint, at Ferozeshah, 21st December. The shot entered at the posterior part of the joint, and appears to have coursed round the bone, and come out on the inner side of the arm, about four inches above the elbow-joint. The arm and fore-arm are wasted and almost useless. He received a sabre wound in the upper part of the forehead. The bone is not depressed.

No. 46. Serjeant John Firby, 3d Light Dragoons, having, on the 21st December, at Ferozeshah, had his horse shot under him; when he was down and endeavouring to defend himself, he received a sabre wound on the left fore-arm. The *ulna* was cut through at its inferior third, and about a month afterwards the lower end of the *ulna* was taken out, probably necrosed. On board ship an abscess formed in the palm of the hand, and portions of the tendons were taken away. He has now entirely lost the power of the fore-arm and hand. He has also a sabre cut on the left side of the head, behind the ear. The bone is not depressed. He received a musket shot immediately above the right knee-joint, and the ball was taken out shortly after.

No. 47. Private Joseph Andrews, 29th Regiment, at Ferozeshah. A grape shot struck the posterior part of the left arm, close to the wrist-joint, penetrated between the *radius* and *ulna*, and made its exit at the ball of the thumb. Several pieces of bone have been taken away. The joints are now ankylosed, and the hand perfectly useless.

No. 48. Private John Gasson, 3d Light Dragoons, at Ferozeshah, 21st December, received a gun-shot wound. The ball entered over the right *olecranon*, passed along the inner side of the fore-arm, and came out about its centre. After the exit of the ball it fractured the thumb; a piece of bone came out at the *olecranon;* the wounds are now healed. The thumb is contracted and bent outwards, causing a large projection on the inner side of the metacarpo-phalangeal articulation, and it is quite useless. He has little or no power over the fingers, probably in consequence of the course of the ball among the flexor tendons.

No. 49. Private Henry M'Green, 29th Regiment, was wounded at Sobraon by a musket shot fired from the intrenchments. The ball entered on the outer and upper part of the left thigh, and fractured the bone obliquely, probably as low as the knee-joint. The ball, he states, has been felt in front of the knee, and was once about to be taken out; it cannot now be felt. One piece of bone came out from the thigh, and one from an opening which formed over the head of the *fibula.* The lower part of the femur is much enlarged, and the tibia is partially dislocated outwards. The left leg is about three inches shorter than the right.

No. 50. Private John Burley, 31st Regiment, at Moodkee, while charging, and when about three yards from the guns, was struck close to the head of the metatarsal bone of the great toe by a musket ball, which passed almost transversely out. Several pieces of bone have come away. He cannot move the toes, nor bear much weight on the foot.

Case of Lithotomy.

A portion of cane nearly four inches in length, forming the nucleus of the *calculus.*

Private James Hussey, aged 25, 18th Regiment; total service $7\frac{5}{12}$ years, of which six months were in Bombay and $6\frac{4}{12}$ years in China.

At Hong-Kong, on the 24th October 1845, he was taken to the guard-room in a state of intoxication, and when he became sensible, he found a piece of cane about a foot long in the urethra, retained by a string tied round the penis; on removing both of which there was slight hemorrhage. The guard-room contained at the time a number of men waiting to be tried by general courts-martial, and he suspected one of them to have played this trick upon him. He did not go into hospital for a month after, when he complained of pain over the pubes, inability to void his urine, and when able to do so, the urine was passed mixed with blood and a copious deposit of mucus. On introducing the catheter a grating sensation was experienced. The patient himself did not know that anything had been pushed into the bladder.

He was admitted into Fort Pitt General Hospital 19th May 1847, very much emaciated, with a dry and scaly skin, gums spongy and scorbutic. He was also labouring under diarrhœa, which attacked him on board ship five months previous, as he was about leaving China. He complained of constant excruciating pain across the bottom of the abdomen, at the site of the neck of the bladder and along the course of the urethra. He could not retain urine, which was continually flowing from him in drops. When collected, it was found to be thick, very fœtid, alkaline, and deposited a quantity of mucus. On introducing a sound, a large stone, supposed to be of soft consistence, was at once detected. The great pain which he suffered for the last eighteen months made him very anxious to have something done for his relief. His general health having considerably improved, and the diarrhœa having been in a great measure checked, on consultation it was determined to remove the calculus, which was accordingly done by the lateral operation of lithotomy, on the 14th June.

Before conveying the patient into the operating theatre, in consequence of his being unable to retain his urine, and the very contracted state of the bladder, some tepid water was thrown in, which came away immediately by the side of the catheter. On placing the patient on the table, and introducing the staff, it was felt to enter the centre of a large soft stone, and there become in a manner fixed. On making the incision into the neck of the bladder a small quantity of fluid escaped. The bladder was now found to be completely filled with a large friable stone, and very firmly contracted in its contents. Having scooped out several large masses, a hard body was found lying transversely, which was also extracted by the finger and scoop; the remainder of the stone having been taken away and the bladder washed out, the patient was put to bed. There was no hemorrhage, and the operation was concluded in less than five minutes.

The hard body extracted was a piece of bamboo, 3¾ inches long and hollow in the centre, what is frequently used by soldiers as a stem for their tobacco pipes. The portion of stone extracted and washed out must have weighed two ounces; the quantity preserved weighs one ounce, and was found to be uric acid.

The patient never had a bad symptom, and although his health was very indifferent, the wound was all but healed in three weeks after the operation; a small fistulous opening, however, continued to give exit to the urine for some time, and required to be touched with a hot wire, after which it closed completely. The diarrhœa has also gradually ceased, and he is now in good health, and will soon be able to resume his duties as a soldier.

Remarks.—Of the six cases of wounds of the head, two are from sabre cuts, two musket or grape shot in the cranium, and two in the face. The case of M'Donnell is interesting on account of the course of the ball among the important arteries and nerves at the base of the skull. That of Willis, in whom the ball still remains in the cranium, without producing much inconvenience or disturbance in the brain, is not less instructive.

Of the five interesting cases of wounds of the lungs, in not one was the patient much affected, or suffered in almost any degree from the course of the ball through this organ; and in the two, May and Haverty, which were examined minutely with the stethoscope, the signs gave reason to infer that the lung was in each healthy, and had regained its normal vesicular structure, and that obliteration of the small bronchial tubes, or condensation, had not taken place to any extent appreciable to the ear along the tract of the ball.

If *post mortem* examinations could be obtained, probably the following diseased appearances would be found. In some cases, the lung would be adherent to the chest only where the ball entered, and also at its exit; and in others, throughout the whole extent of the pleural sac. On making a section of the lung, a narrow white line or cicatrix, resembling an obliterated bronchial tube, would be seen indicating the course of the ball. In the case of May (No. 7), where the ball remains in the lung, a cyst has in all probability been formed round it, and therefore preventing it from causing much disturbance to the functions of the respiratory organs. On a subsequent examination of this man's chest, exactly over the spot where the ball entered, there was heard very distinctly a peculiar whistling sound, which continued for ten or fifteen minutes; yet, on removing and reapplying the stethoscope, it could not again be heard. This sound may be explained by supposing the ball to be situated in a cyst, and at times air may enter the cavity, and cause the sound mentioned; and by some movement in the lung, the position of the ball may be changed so as to fall against and block the air-tubes which open into it. The man states that he also occasionally hears this sound.

Fletcher (No. 12) was in perfect health until immediately after he had taken a full meal, which no doubt he had frequently done, since the time the wound had been inflicted. It may therefore be asked what was the immediate cause of vomiting and the sudden attack of illness. This question it is difficult to answer in a satisfactory manner. Probably the portion of the stomach still in the abdomen, having become overdistended, spasmodic contraction of the diaphragm may have been induced, and prevented the food from entering the larger portion of the stomach situated in the thorax. It might nevertheless have been supposed, that after the contents of the lower portion had been ejected, the irritability of this organ would have ceased, which, it has been seen, did not take place, but continued to the last without the slightest abatement. The reason for supposing that it was spasmodic contraction of the diaphragm is, that on examination, the opening in this muscle was found large enough to admit two fingers with ease along the side of the gut.

Although it was known that he had been wounded in the side, yet it was never suspected that the wound was in any way connected with the disease for which he was under treatment. His general appearance was most peculiar; the contracted state of the abdomen, the total absence of pain in the belly after the first day, and of all inflammatory symptoms, and the occurrence of pain in the left shoulder. It is known that, in diseases of the liver, pain in the right shoulder is a common symptom. The pain in Fletcher may have had the same origin, whatever that may be, as no satisfactory explanation has as yet been given of it, except that it may depend upon some irritation of the ramifications of the phrenic nerve, communicated along its course to its origin from the cervical and brachial plexus, and then reflected by twigs distributed to the shoulder.

A number of the symptoms which presented themselves in this case were those usually attendant on strangulated, inguinal, or crural hernia, viz. incessant vomiting and obstinate constipation, a small and wiry state of the pulse; but there was no stercoraceous vomiting, dissection showing that it could not occur. The intussusceptions were the first morbid appearances seen on opening the body, and the remark was immediately made, that they did not present the appearance of invaginations which were the cause of death, but rather what are sometimes seen to have taken place during the latter stages of life, where the patient had no symptom of derangement of the intestinal canal. It is therefore most probable, that the intussusception took place after the commencement of the fatal disease, and were not the cause of it, but to be attributed to the violent peristaltic motions, induced by the action of the large doses of croton oil which were administered.

At first view of the case, it appears to throw some light on the physiology of vomiting, as to how far the muscles of the abdomen and diaphragm co-operate with the stomach in this mechanical act. But it must be remembered that the whole of the stomach was not displaced into the thorax, and therefore whatever influence they possess may have been exercised upon the part in the abdomen. It is also to be supposed that the contents of the stomach and transverse arch of the colon situate in the thorax remained the same from the time they became strangulated.

Mr Guthrie states, in his work on Gunshot Wounds, note to page 14, that "wounds of the diaphragm, in consequence of the motion of the parts, never unite, but always leave an opening with rounded edges, through which hernia of the stomach or intestines are apt to be formed, and sometimes become strangulated." This may be the fact generally; but in a case where the wound of the diaphragm is small, and the patient kept quiet and on low diet, there is no good reason for supposing that the wound would not unite; the base of the lung might adhere to the diaphragm, close up the opening, and so form a boundary between the thorax and abdomen, and thus prevent a hernial protrusion. Besides, it is only in those cases where a hernia has taken place, that a surgeon is led on autopsy to a careful examination of these parts. I am therefore disposed to think, that in cases of wounds of the lower part of the thorax, the diaphragm has been injured in many instances, and the patients have so completely recovered from its effects, as never after to draw any attention to it, and that union of the divided fibres of the diaphragm, or pleuritic adhesion, takes place in some cases, and prevents a hernia from being formed.

It is very remarkable, that this extraordinary malposition of the stomach and colon should produce so little inconvenience to the patient, and that in every respect Fletcher was in perfect health until strangulation took place. The case of Sergeant Dennis Barry, who was wounded in nearly the same situation, where there was also a hernia of the stomach and colon into the thorax, and who, notwithstanding, continued to do his duty as a soldier for nearly 22 years, and ultimately died of gangrene of the left lower extremity, shows how little inconvenience he likewise experienced, and how little effect, under certain circumstances, this displacement produces. The parts forming the hernia in this case are preserved in this museum.—Preparation, No. 1153.

Had the exact state of things been accurately ascertained during life, what steps could have been taken for his relief? Would a surgeon be warranted in cutting into the abdomen and reducing the hernia? In this case it would have been impossible, on account of adhesion of the displaced viscera to the surrounding parts. The diagnosis of such a displacement would be extremely difficult, if not impossible.

There is at present in this hospital a patient wounded in nearly the same situation. Private Thomas Eathy, aged 25, who was wounded on the 29th December 1843, at Maharagpore, by a grape shot, which entered about three inches below and to the outer side of the left nipple, between the eighth and ninth ribs, and was cut out shortly after the accident, behind and at the angle of the eleventh rib. He suffered from pleuritis of the left side in 1844. His general health is now good, and he has no complaint except shortness of breathing, or rather, uneasiness in his chest when wearing his knapsack and cross belts. Nothing abnormal can be detected in the chest. I have no doubt that the diaphragm, in this case also, was wounded; and if so, the wound may possibly have closed, or pleuritic adhesions may have been formed, and prevented a hernial protrusion. If, however, Eathy was seized in the same manner as Fletcher, after all other means had failed, and after a careful examination, I think a surgeon would be justified in cutting into the abdomen, and trying to reduce the hernia, if one existed.

It is possible, in the case of wound of the abdomen, that the intestines were not seriously injured; the ball, however, seems to have entered the peritoneal sac, and to be now lodged in the pelvis.

Twelve cases of amputation by the flap, and eleven by the circular methods, are given, which show the superiority of the former over the latter. In almost all, by the circular mode, the skin is adherent to the bone, which renders it very difficult and troublesome to fit any apparatus to the stump, even where there is no direct pressure made on the end of the bone; the limb being merely placed in the wooden box, the adherent skin was dragged and put upon the stretch, causing much annoyance and inconvenience to the patient. The flap method certainly appears to be the best, more especially in military surgery, where rapidity of execution must be very desirable on the field, when so many operations are to be performed with as little delay as possible; and the foregoing cases show the result to be very much in favour of the flap, on account of the better cushion of muscular fibres preserved, which protects the bone, and lessens the chance of injury from pressure. Neither does secondary hæmorrhage occur more frequently in the flap than in the circular method; and when the artery is cut obliquely, if the ligature be placed high on the vessel, there can be no reason to dread such an occurrence. The other arguments generally urged in support of either operation by their respective advocates, are, I imagine, much upon a par, and therefore do not require to be noticed.

The cases under the head of contractions and ankylosis are generally the effect of musket wounds in and near joints, and are interesting as showing the condition of the limb after such injuries.

The case of lithotomy presents features requiring a few remarks.

It is difficult to conceive how a portion of cane nearly four inches long could have been forced into the bladder without the patient being aware of it at the time, or even afterwards. When once in the bladder, it might have been expected that the irritation would have caused contraction to such an extent, that ulceration of the coats of this viscus would have taken place. The patient did not suffer much annoyance until a month after its introduction, when he went into hospital, and a grating sensation was then felt from the deposit around the foreign body; but there was not the slightest suspicion of a piece of cane being present.

At the time the operation was performed, the patient was far from being in a favourable state. His constitution was shattered from service in China, dysentery, and the irritation produced by the calculus. The latter was removed, as affording the only chance of preserving his life, and as no farther advantage could be expected from delay.

On cutting into the neck of the bladder, it was found impossible to introduce the finger, from the contracted state of the bladder and the size of the stone, and therefore it could not be removed by the forceps. The scoop was the only instrument which could be employed.

If the deposit around the foreign body, instead of being composed of uric acid of a soft and friable consistence, had been hard and compact, such as oxalate of lime, its extraction would have been impossible, on account of the contracted state of the bladder not admitting of the introduction of the forceps. The *calculus*, however, might have been broken by a lithotrity instrument. Still the portion of cane, with fragments of the calculus attached to it, could neither have been broken nor removed without being first cut across; but as the existence and nature of the nucleus was not known, it is easy to imagine the difficulties which the surgeon would have had to encounter.

Fort Pitt, 23d August 1847.

ART. IV.—*On the Period of Puberty in the Negro.* By JOHN ROBERTON, Surgeon, Manchester.

THAT portion of the British West Indies lying within the tropics consists of Jamaica, the Windward and Leeward Islands, Honduras, and English Guiana. These islands and territories stretch through twelve degrees of latitude; the most southerly portion of Guiana being in latitude 6°; the most northerly point of Jamaica reaching latitude 18°.36′.

The climate varies in different islands and on the mainland, according to the degree of elevation above the sea; to the presence or absence of dense forests and marshes; and to other circumstances which need not be enumerated: only it is worthy of notice, that, on the level of the sea, the temperature for the year differs very little in different places, notwithstanding the considerable range in latitude. Nowhere, either in the islands or on the mainland, does the mean temperature exceed 82°, or fall below 79°.* In regard to the climate of Demarara, the most northerly portion of an alluvial tract, lying between the mouths of the rivers Oronoco and Amazon, a friend writes me in these words:—"I keep a thermometer in my study, which is the coolest room in the house. It is, too, perhaps one of the coolest in the colony. The range I have found to be from 76° (the lowest I ever saw it, at the coolest season of the year, from January to March, and at the coolest hour of the twenty-four, between five and six in the morning) to 94°, the highest I ever remember to have seen it rise. In the next room, which is exposed to the afternoon sun, it often rises above 100°. For the last three months, or since the 1st of August, it has risen generally to 87° during the day, and fallen to 81° towards morning. I should say that the average temperature of my study from January to January is not lower than 81°, nor higher than 82°." When such is the mean temperature of a room removed from the direct influence of the solar rays, and supposed to be one of the coolest in the colony, it will be admitted that the climate of Demarara is truly tropical. With this we may compare the highest mean for Europe, namely, that for Cadiz, which is only 62°.†

The peasantry of these colonies, it is well known, are negroes—a people who have been imported, at different times during the last three centuries, from shores directly facing their present abodes. Let any one trace on a map the winding coast of West Africa, from the mouth of the Senegal, latitude 16° north, to the equator, and he will have before him the prolific countries which, for the most part, have yielded those cargoes of slaves, the progenitors of our black fellow-subjects. The peasantry of the West Indies, therefore (of whom alone I am speaking), not only now inhabit tropical regions, but were originally from countries in the same latitudes —a circumstance entitling them to be considered an intertropical race, in a sense more exclusive perhaps than any other extensive group of the human family. The Hindus, the Chinese, the Arabs, the Indians of equinoctial America, may all, with some appearance of probability, be shown to have a northern origin. But this cannot, as matter of history, be affirmed of the negroes, who, from the earliest times, are known to have inhabited tropical Africa. This fact, in regard to the West Indian negroes, merits particular attention, because, if fervour of climate operating through thousands of years—if a climate not only hot, but the yearly mean temperature of which is as high as that of any other known, does indeed cause early puberty, then here we may expect it to be found. In this quarter of the world the controversy may be brought to the test. Other causes than heat *may* perhaps produce early puberty; but if heat of climate be, as is alleged, a uniformly powerful cause, irrespective of all others, *here* the effect will assuredly be found. I dare not affirm that my researches thus far will suffice, in the estimation of the profession, to settle the question, because they have been prosecuted with smaller success than I had anticipated. I have found it difficult indeed to awaken the curiosity of my West Indian correspondents so far as to induce them to assist me in the collection of facts. It has been easy to obtain *opinions* from numbers; but thus far I have not been fortunate enough to obtain the co-operation of many persons willing, as was the case in Hindustan, to encounter the trouble of collecting a body of particular instances. What I have now to produce will be found of value—some may think it even decisive. I should be glad, however, to found my deductions on a more extensive collection of individual facts than I have it at present in my power to bring forward. And one object—the main object I have in view in the publication of my brief paper—is to awaken attention among the West Indian readers of the Edinburgh Medical and Surgical Journal, to this unsettled controversy, relative to a point in the natural history of man, of confessedly the greatest importance, which a little zeal on the part of even one or two among them might easily put to rest for ever. Even *one* professional man, of the industry and curiosity of my correspondent in Madeira, Dr Dyster, might, with the help of his friends in (for example) Demarara, or one of the more considerable islands, accumulate, in the course of twelve months, a table of facts sufficient for this purpose. Some of my correspondents hint at various impediments in the way of such an inquiry among the negroes. But pleas of this nature are of little weight; for in no quarter of the world is an investigation of the kind alluded to either easy or agreeable, as those who may have made the attempt will testify. The alleged *peculiar* impediments and difficulties in the West Indies, I regard as being ideal or exaggerated. I speak *advisedly.* Indolence and want of curiosity, on the part of a portion of the medical profession there, constitute the only real obstacles.

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* On Sickness, &c. among the Troops in the West Indies. By Captain Tulloch. Journal of Stat. Society, Vol. i. page 130.

† Also with the annual mean temperature of the hottest portion of the peninsula of India, which Dr Geddes, in his "Clinical Illustrations of the Diseases of India, &c." gives as between 79° and 80°. This is from observations in the years 1829, 1830, and 1831, made at Masulipatam and Kamptee, lat. 16°. The mean temperature of the West African shores, within the tropics, is between 81° and 82°. This is the case at Sierra Leone, lat 8°. 30′.

Your readers will find in a paper, in No. 157 of this Journal, a few facts regarding the age of puberty in the negro obtained from Jamaica and Antigua. Two parties in the former island, reporting separately, state that no case of menstruation in the negress before twelve had come under their notice; and a physician of Antigua, after twenty years' experience, writes to the same effect. This testimony I shall again have occasion to notice.

The additional information I have to produce was received chiefly in the course of 1845, but has been kept back till now, owing to the hope I was by correspondents led to entertain, of large accessions to my stock of facts, to have reached me before this, both from Demarara and several of the islands. As nothing, however, has resulted from the delay, I now (as already said) lay before my professional brethren the few materials in my possession, in the expectation, I confess, of thereby still tempting some of the more zealous among them in the colonies, to aid me in obtaining a body of evidence, such as may leave, in this portion of the field, nothing further to desire.

A missionary, whose letter is dated Demarara 1845, writes, that he has been in that colony eleven years, and has taken notice, with respect to negro girls residing in his own family, that " they have begun to menstruate in their fifteenth year;" adding a number of particulars tending to show that this is about the age in general when the change occurs in the negress.

Another missionary, in Demarara, writes (November 1845), " In reference to the age at which females of the African race begin to menstruate in Guiana, my wife and I have had about twenty cases before us, several of whom were under her care [instruction] for a considerable time, and at the interesting period of life referred to; and, of this number, we could only find one case before the age of fourteen, and even in this instance, there was some uncertainty as to the age of the individual. In the majority of cases the age at which this change took place was considerably later than fourteen years." He adds, "I have consulted T. P. Watts, Esq., M. D., my medical adviser, a person of high respectability both as a man and a physician, who, after twenty-eight years of observation and experience in Guiana, declares it to be his belief, that the period of menstruation in African females in Guiana, is much the same as in European females living in Europe, being in all ordinary cases between the ages of 14 and 17, or even 18 years. He had met with one instance of a girl, a mother at 14, but one only; and he regarded that as a case altogether extraordinary."

By another missionary in Demarara, I am favoured with memoranda, furnished him by two medical men of his acquaintance, both, as in the case of Dr Watts, long resident in the West Indies. The opinions of the first gentleman will have not the less weight, that their author retains the common opinion, that females arrive at puberty earlier in a warm than a cold climate.

"With reference to your query concerning any difference that may exist as to the period of puberty between the black and European, after twenty-three years' experience in the West Indies, twenty of which were passed in British Guiana, I am of opinion that there is no difference in the black when compared with the European (if by European be meant the *white* born in the West Indies), there being, evidently, a difference as to their maturity in the residents of a warm and cold climate, but not, I apprehend, confined to the negro or black race.

"The average period of puberty in the black, in the West Indies, I take to be from 15 to 16, although I have known individual cases in which both black and white have become mothers at 15½; but these cases I should consider as exceptions."

G. Bannici.*

The second writes as follows:—"*British Guiana, August* 6, 1845. From an experience of above twenty years' practice in the West Indies, I am of opinion that the animal developments in general, and the sexual ones in particular, are precisely the same here as in Europe, and similar in all the races, whether natives or foreigners. I do not think that menstruation occurs earlier here than in Europe; but its arrangements are somewhat dissimilar, not being those of difficulty, pain, or scantiness, but of entire suspension, or too great profuseness, or of a bi-monthly-recurrence. This latter affection is very common, affecting indiscriminately all colours, and both married and single. It is true that sexual intercourse takes place much earlier among the Indians, the negroes, and the lower classes of the mulatto population, than it does in the northern parts of Europe. This, however, is not the result of earlier puberty, but is an anticipation of nature, arising from moral, and not from physical causes. As a corroboration of these opinions, I may notice, that I have never observed the protrusion of the mammæ (nor growth of hair on the pubes) to occur, except in a very insignificant number of cases, before the period between 14 and 16.

"I may remark, that the earliest period at which I have known menstruation to commence was at ten years. The individual was a native, the offspring of European parents, and a remarkably healthy and uncontaminated child. She is now 20 years of age, and has never suffered from any disturbance of the function so early established."—C.

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* The autograph is so illegible, that I am not sure I have hit on the name.

I have next, through a respected friend in Tobago, a valuable communication from Dr Campbell Stirling of that island, dated Windsor Cottage, August 6, 1845. The following are extracts containing what more particularly relates to the period of puberty.

" Query 1. At what age does a negress begin to menstruate?

Ans. From 13 years of age to 17. The average I should estimate at about 15. I have known some as early as 11, and others as late as 20; some never. I do not think that the relaxation of a warm climate has much influence over the period of menstruation in reference to its commencement."

" Query 2. At how early an age have you known a negress to give birth to a child? and what is the usual age when a first birth occurs?

Ans. Much the same as in Europeans, and much ruled by their morality of conduct; seldom earlier than 16."

" Query 3. What is the most advanced age at which you have known a negress give birth to a child?

Ans. Much, again, the same as in Europeans—between 40 and 45, or even towards 50."

" Query 4. What is the age at which menstruation ceases in the negress?

Ans. From the age of 45 to 52."

Dr C. Stirling afterwards adds what shows that he does not perhaps altogether renounce the prevailing belief as to the influence of cold in retarding puberty. His words are, " Although I do not think that tropical heat *generally* causes premature female adolescence in negresses, it often helps to bring on premature decay; and I believe the same holds good in the other sex. In fact, I am of opinion, that the sons and daughters of a well regulated black family, will be prepared for the functions which providence has assigned to them about the same period of life as a European family in the same circumstances—except, perhaps, in the very coldest latitudes."

The most valuable of the communications I have yet received is one from Barbadoes (including a table), from the pen of a medical gentleman, Mr G. W. Bowen, a native of that island, addressed to a mutual friend, a respected missionary. The following is an abridgment of what precedes the table. The date is 3d October 1845:—

" In answer to a letter from Mr Roberton, handed me some months ago, concerning the influence of climate on puberty in the negress, I have to apologize for my delay in replying, which has principally arisen from the difficulty experienced in ascertaining *the exact age* of the parties applied to, particularly the older ones. This difficulty will account for my having furnished only twelve cases with reference to the age at which menstruation ceases. I can inform Mr Roberton, that the evidence from the Jamaica and Antigua missionaries (alluded to in his letter) is perfectly correct as respects Barbadoes also. While I have been engaged in researches among the blacks, I have obtained information among the females of European descent, such as bears me out in asserting that the period varies alike in either race; and a table of cases taken promiscuously from either would present the same variations as to age. The only differences between the negro and the white in Barbadoes are colour, features, and, in general, bodily strength—the former being more hardy and better adapted to the climate. The want of education may be also said to make a difference as to the moral character of the negro; for, generally speaking (there are some examples to the contrary), the habits of the negroes, both male and female, are very depraved. The females are corrupted at a very early age. The consequence is, that as soon as the period of puberty arrives they conceive, and, on this account, they may be said to bring forth children at an earlier age than the women of European descent."[*]

To Mr Bowen's table, containing the ages of 64 negresses at the period of puberty, are here added the 13 cases from Jamaica, to be found in my former paper, making together 77.

Ages.	Barbadoes.	Jamaica.	Both.
11	1	...	1
12	1	2	3
13	4	3	7
14	8	4	12
15	10	3	13
16	15	1	16
17	12	...	12
18	7	...	7
19	5	..	5
20	1	...	1
Total, .	64	13	77

Mean age, 15 years 7 months 10 days.

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[*] A friend, now resident in England, but a native of the West Indies, and long occupied as a planter, in a letter assures me that the tendency to depravity is stronger "in the mixed or European and African progeny, and the white creole, than in the black." After some other interesting information on the state of morals in Antigua and Jamaica, and the prevalence of concubinage, he continues: "I have heard of well authenticated cases, where the most brutal tyranny has been exercised for the accomplishment of sexual intercourse with young females above the age of puberty, by those who had them entirely under control, without the desired effect. Mothers have made traffic of their daughters' virtue, in many instances, at an early age—I should say not under 15 years—to obtain a present price, and to secure influence, but the daughters have had to be dragged

This mean age, of fifteen years seven months and upwards, is later somewhat even than that in the lower class of women in England, but not much. It, in fact, agrees substantially with the results of my inquiry amongst the patients of our lying-in hospital, given in the North of England Medical and Surgical Journal for 1830; and with those in Mr Whitehead's recent work on Abortion, &c., obtained chiefly from the same source.* In a word, the black peasant females of the West Indies, the peasantry of Madeira (of Portuguese origin), the Esquimaux of Labrador, and the manufacturing females of Manchester, would appear to arrive at puberty about the same period of life, viz. the middle of the sixteenth year. The more comfortable classes of society menstruate at a somewhat earlier age on the average, which, however, is, of course, to be ascribed to no natural difference, but to the more propitious circumstances in reference to health.

The following is a summary of the testimony, regarding the age of menstruation in the negress, of the different medical men with whose communications I have been favoured:—

Dr T. P. Watts, Demarara, .	from 14 to 17 or 18 years.
Mr G. Bannici, do. .	15 to 16 years.
One who signs " C." do. .	14 to 16 do.
Dr C. Stirling, Tobago, .	13 to 17, mean " about 15."
Mr Bowen, Barbadoes, .	mean 15 years 7 months.
Dr Nicholson, Antigua, .	14 or 15 years.

The evidence supplied by several experienced missionaries, aided by their wives, upon which I do not, however, desire to lay undue weight (though I think it well entitled to regard), corroborates in every respect the testimony of the professional witnesses.

The period when menstruation ceases in the negro corresponds, again, to what we find among the working people of this country, namely, from the 45th to the 55th year. Mr Bowen supplies the ages in twelve cases: the years 47 and 48 have each one case; 49 has three; 50 and 51 each two; 52, 54, and 55 each one. Dr C. Stirling states the period in Tobago to be " from the age of 45 to 52."†

The earliest age at which the negress is known to give birth to a child is sufficiently shown by the several witnesses. Dr T. P. Watts, after twenty-eight years' experience in Demarara, had met

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or driven to their seducers more frequently than otherwise." I could not withhold this testimony of a credible witness in favour of the native worth of our long wronged but now emancipated fellow-subjects of the negro race, especially as it is so contrary to the general impression we have been led to entertain. I have full confidence, moreover, in the above testimony of my friend, as there could hardly be found a person more thoroughly conversant with the condition of West Indian society.

* Mr Whitehead, from 4000 instances, calculates the mean age at fifteen years and seven months nearly.

† See this Journal, Vol. xxxviii. No. 113, for the age when menstruation ceases in England.

with one instance of a girl a mother at 14, but one only; Mr G. Bannici had known the occurrence at the age of 15, but considered such cases exceptions; Dr C. Stirling seldom earlier than 16; and Mr Bowen at 15. On the coast of Labrador, amongst the Esquimaux, Mr Lundberg has known a first child brought forth about the same age—15¾ years.*

I have now produced the whole of the evidence in my possession, relative to the natural history of the menstrual function in the West Indian negress. I will not at present farther attempt to estimate its importance, whatever that may be. Readers will judge for themselves. Should what may seem a conclusive body of facts reach me after a while, I may, on making it public, feel that the time is arrived, when I can with propriety review the whole subject of my researches, and consider in detail those physiological, moral, and social questions which the inquiry concerning the age of female puberty in different races and climates obviously involves. Once more, however, I repeat—the influence, if any, of tropical heat on the period of puberty, may easily be decided among the negro race of the West Indies. And I again respectfully invite professional gentlemen in those colonies, whose turn of mind may dispose them to take part in such an investigation, and whose position may furnish opportunities, to give me their assistance.

ART. V.—*Case of Dislocated Head of Radius successfully reduced two years and one month after the occurrence of the Dislocation.* By JAMES STARK, M. D., Fellow of the Royal College of Physicians of Edinburgh.

MARY H., about 9 years of age, while playing with the nursery maid on the first week of May 1845, fell from a table against the fender, the elbow of the right arm coming in contact with its edge. Acute pain, with immobility of the joint, speedily followed by great swelling, ensued, for which the family surgeon was called, but considering it as a simple contusion, treated the matter lightly, and merely applied camomile fomentations. About a month after the accident, when one of our more celebrated surgeons was calling at the house, he was shown the child's arm, and after examining the elbow-joint, pronounced it to be "a case of fracture of the neck of the radius—a very rare case—for which, however, at that distance from the date of the accident, nothing could be done."

When called to attend this child for an attack of chicken-pox, on the 4th May 1847, the disfigured form of the elbow-joint attracted my attention. I then learned that, from the date of the accident, the child had never regained the free use of the arm, that it was a constant source of annoyance to her, as she could not allow it to be handled freely when at play with her companions, nor could she lift any weight, or carry even her bag of school-books in that hand; in fact, she carried the arm as if it were paralytic. The moment the arm was examined, a dislocation forwards of the head of the radius was recognized—a dislocation by no means common, yet one whose diagnosis is so distinct that it is surprising how it could evér have been overlooked. The hand was pronated, and it gave uneasiness to supinate it. The arm could not be extended quite straight, but when extended, the rounded head of the radius, drawn up by the biceps tendon, was seen to rise and form a distinct prominence in front of the elbow-joint, and was both seen and felt to rotate, as the hand was alternately pronated and supinated. When the arm was bent it was suddenly arrested by the head of the radius coming in contact with the fore part of the humerus. From practice, and probably also from the bones having somewhat conformed to their abnormal situation, the hand, when the arm was fully bent, could be made to touch the chin with the tips of the fingers. The head of the radius was felt to be wanting in its usual situation—a hollow space filled with a soft, but elastic, apparently fibrous deposit, existing at the outer and back part of the elbow-joint, where the head of the radius ought to have been felt articulated with the os humeri. As the radius could be traced continuously to the rounded head which projected in front of the elbow-joint, it was quite apparent that the head of the bone had been dislocated, and not fractured. The round head seemed almost as large and as little altered in figure as if it had been newly displaced. It was otherwise, however, with the external condyle of the humerus. It was greatly diminished in size, and its sharp edges much rounded off—facts very notable when compared with the corresponding condyle of the sound arm.

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* See former paper respecting puberty in Labrador.

Two questions then presented themselves for consideration. *First*, Was it prudent to meddle at all with such an old dislocation? *Secondly*, If so, and the reduction should be effected, was there any chance of the restored joint proving as serviceable as even the preternatural one now formed, seeing that the articular surface of the humerus was so altered in shape?

On consulting the writings of Flaubert, Marx, Sir Astley Cooper, and Dupuytren, who more especially treat of the reduction of old dislocations, it was ascertained that these able surgeons coincided in opinion, that ball and socket joints could be reduced at a much later period of time after the occurrence of the dislocation than hinge-joints; and yet, of even these joints they limit the period, during which it is either safe or possible to reduce them, to a few months. In the case of hinge-joints (as the elbow) they unanimously agree, that within a very short period after dislocation, they become irreducible, without using such an amount of violence as to tear across muscles, nerves, and blood-vessels, and thus endanger life by rendering amputation of the limb necessary.

With regard to the particular dislocation now under review, Sir Astley Cooper relates, that during his extensive practice he had only seen six cases. Of these, four were unable to be reduced, though the most varied and violent efforts were used. The other two were reduced. In my own practice, only two cases of dislocation of the head of the radius forwards had previously been met with. In the first case, the woman fell over a window three stories in height, and, besides other injuries, received a compound fracture of the condyles of the humerus, with displacement forwards of the head of the radius. In consequence of the injury to the soft parts, and the fracture of the condyles, the head of the radius could not be retained *in situ*, and when the cure was effected, the *callus* had formed a new socket for the head of the radius, and it was not considered prudent to interfere with it. From the head of the radius moving in a bony socket, the arm soon afterwards became quite strong, though it never could be bent so as to allow the fingers to touch the face; and the woman, by occupation a washerwoman, was able to prosecute her usual employment. In the other case, the accident occurred from falling on the ice, alighting on the hand; and the reduction was easily effected by pulling by the hand, and bending the fore arm on the arm, when the head of the radius was brought to the edge of the articular surface of the humerus, at the same time pressing down the head of the radius with the thumbs.

In the present case, the child was of a soft lax fibre, and as the arm was comparatively useless and weak, it was wasted or much softer, thinner, and less muscular than the sound arm. Provided, therefore, that the extension was applied gradually, and care was taken not to employ such violence as would cause the rupture of muscles, nerves, or blood-vessels, there appeared to be nothing to contraindicate an attempt at reduction. The consideration of the second question seemed still further to indicate that an attempt ought to be made to reduce the dislocation. The present state of the arm was a constant source of annoyance. It was disfigured in shape, was weak, and comparatively useless. As the head of the radius was uninjured and normal in form, it was not considered any objection that the external condyle of the humerus was somewhat altered in form, seeing that it was probable that when the bones were put *in situ*, that part would recover its former size and structure. By effecting the reduction, therefore, there was every chance of the limb being rendered more useful than it had been since the period of the dislocation.

As from the cases related by Flaubert, Marx, and Dupuytren, it appeared that the chief danger in reducing old dislocations arose from the risk of rupturing the muscles, blood-vessels, or nerves, by employing violent efforts at reduction, it was determined in this case to extend the arm firmly, but gently, day by day, till the new adhesions of the head of the radius were so much lengthened, or the head so loosened from its new site, that, by the employment of not much additional force, the bone would at last be put into its proper place. The extension was effected by seizing the hand of the patient with the right hand, bending the elbow-joint so that the fore-arm formed a right angle with the arm, and applying the counter-extension by passing the left hand close above the elbow-joint, and thus fixing the humerus. The extension was then continued till slight uneasiness was complained of. This was repeated daily from the 12th May till the 2nd of June 1847. By this time the head of the radius had become quite loosened, and could be pulled to the edge of the articular head of the humerus. On the 2d of June, therefore, I made the extension and counter-extension myself, and when the head of the radius arrived at the edge of the articular surface of the humerus, allowed the ball of the thumb of the left hand to press against it; when bending the fore-arm on the arm, the bone quietly slipped into its place. The head of the radius was found to have a strong tendency to slip out of its place and return to its former situation in front of the elbow-joint, partly on account of the altered form of the condyle of the humerus, partly from the cavity of the joint being filled with the forementioned elastic fibrous deposit. The arm was therefore kept much bent, and the hand tied up across the chest, so that the tips of the fingers almost touched the opposite clavicle.

Slight tumefaction, increased heat, and redness, with uneasiness rather than distinct pain, followed. These were speedily relieved by an evaporating lotion, but no other treatment was required, and the child was allowed to take exercise as usual. After a few days, a bandage was applied round the joint, but gave such uneasiness that it was obliged to be removed. No application was therefore made to the joint itself. After the lapse of about a fortnight, slight motion was daily given to the elbow-joint, without, however, lowering the hand or extending the arm. In about three and a-half weeks after the reduction, the fore-arm was lowered so as to form nearly a right angle with the arm, greater motion was daily allowed to the elbow-joint, and frictions were also employed to promote the absorption of the fibrinous deposit, which, from filling the joint, still prevented the close coaptation of the bones. By the 10th of July the motions of the elbow-joint were found on trial to be nearly as extensive as those of the sound arm. The fore-arm could be bent on the arm so as to allow of the hand being put to any part of the head; but when the arm was extended, the head of the radius threatened to rise and resume its abnormal situation. The arm was therefore so tied that it could not be extended further than a right angle.

By the 1st August, or two months after reduction, the head of the radius was found to have acquired such a degree of firmness in its proper site, that all bandages were removed, and the arm was allowed to be free. From this day the hand and arm improved in strength, so that, by the middle of September, both the child and her parents acknowledged she had much more use of the hand than she had had from the day of the accident, two years and four months past.

When examined in October, the cure was found to be nearly perfect. The arm and hand were nearly as strong as the left or sound arm. She could carry freely in that hand her bag of books to school, which she could not previously do, and could lift weights and use every freedom with it.

The elbow-joint itself still presented some peculiarities. The head of the radius, in consequence of the elastic fibrous deposit which filled the joint, was still not in close contact with the articular surface of the humerus, some of this deposit seeming to intervene; and it was also thrown, from the same cause, further forward than natural on the fore part of the elbow joint. The external condyle of the humerus, though perceptibly larger than before the reduction, had not yet acquired its normal form, and the arm could not be extended quite straight in consequence of the contracted state of the biceps muscle of the arm—a contraction produced by the head of the radius lying for upwards of two years in front of the elbow-joint. All the motions of the joint, however, were as perfect as in the sound arm, and complete pronation and supination of the hand were effected with ease. The elbow-joint could be bent so that the hand could touch the shoulder or any part of the head or face. There is every prospect, therefore, that in no long time the elbow-joint will return to its normal state; that the head of the radius will sink to its proper position in proportion as the fibrinous effusion becomes absorbed; and that the biceps muscle again becoming lengthened, will permit the arm to be fully straightened.

This case appears to be important on many accounts. *First*, It shows, that, by judicious management, a dislocation may be reduced at least two years and one month after its occurrence, and this, too, in a child, in whom the bones change more rapidly than in an adult; and it is quite possible that this same dislocation might have been reduced with safety at a much more distant date. *Secondly,* It shows that the mere circumstance of the bones being somewhat altered in form, or the site of the old joint being more or less completely filled up, should not be considered as a bar to the attempt at reduction; for, in this case, the external condyle was much reduced in size, and altered in shape, and the site of the old joint was quite filled up; so that, even when restored to its place, the head of the radius could only be kept *in situ* by keeping the arm much bent. *Thirdly,* It proves the great advantage of making the extension slowly and gradually, and never continuing it so long as to give rise to reaction, but each day merely so to act on the new-formed artificial joint, as to loosen its attachments, and lengthen the muscles, nerves, blood-vessels, and ligaments; so that, when at length the final pull was given, none of these were torn across, and so no risk was incurred of losing the joint, limb, or life, by the incitement of dangerous inflammatory action.

If these principles were applied to the reduction of old dislocations, we should not hear of so many failures, still less of the occurrence of those dangerous lacerations of the soft parts, which have led in some cases to the loss of the limb, in others, to that of life itself. In attempting the reduction of old dislocations, nothing should be attempted by sudden violence, or by one or two violent efforts at reduction. The only justifiable mode of reducing old dislocations appears to be to act on them slowly and gradually, and be satisfied if we succeed in restoring the joint to its natural state at the end even of several weeks.

21, *Heriot Row,*
9*th November* 1847.

ART. VI.—*Report of some cases of Molluscum Contagiosum, with Observations on its General History and Pathology.* By RICHARD PAYNE COTTON, M.D., Member of the Royal College of Physicians, London.

THE subject of this account is a family consisting of the father, mother, and six children (four girls and two boys), of the upper class of poor, but all particularly cleanly and intelligent. The disease first showed itself about four months back, upon the hands and arms of the eldest girl, then 14 years of age, subsequently upon the arms, chest, and thighs of the youngest (an infant of six months old), and after a period of three months, on the head of another daughter, and the arms and chest of the mother, the father, two sons, and one daughter having been entirely free from its attack. The mother recollects that something of the same kind affected her when a child, but it disappeared without interference.

In every case it began in the form of prominent rounded movable tumours, of the size of a small pin's head, gradually increasing to that of a pea, and soon presenting a small central depression, not unlike that of a small-pox pustule. These small tumours are covered merely by epidermis, and at first are soft, red, and shining; but after a period of three or four months, become hard from thickening of the cuticle, and were it not for the remains of the central depression, might be mistaken for ordinary warts.

The surrounding skin is always free from any signs of irritation, and the sole inconvenience attending them, arises from their position, for unless irritated by friction, they produce neither pain nor itching. They are generally arranged in groups of from two to five or six, which never coalesce, and their attachment occasionally pedunculated, is more frequently by a broad base; the greatest number on either of the patients does not exceed thirty. During their early stage, an opaque-white or yellow, cheesy-looking inodorous matter exudes from the central point upon pressure; but as the disease advances, the contents of the tumour become hardened, and distinctly lobulated, and a puncture becomes necessary for its removal. The secretion, under the microscope, presents the following peculiarities.

It is composed of two distinct elements, with an intermixture of granules and epithelium. The stromal substance consists of irregularly waved fibrils, crossing each other in all directions, resembling the white fibrous element; the intra-stromal is formed of spherical or elliptical cells more or less granular, but generally devoid of nuclei; the cells, if separate, are very regular in form, but when in masses, assume a polygonal figure from compression. These appearances are shown in a drawing in my possession; the same character is presented by the secretion in all its stages, but the structure is more compact, and the cells more compressed when it becomes indurated.

The small central depression of the tumours, evidently formed by the closed opening of a duct—the lobulated form of their contents, and their absence on those parts where the sebaceous glands do not occurr—as the soles of the feet and palms of the hands—indicate these organs as the seat of the disease; but the different character of inspissated sebaceous matter, shows that molluscum is not produced by a mere retention of their natural secretion. If the contents of one of the comedones, from any part of the body, be examined under the microscope, it will be found to consist of a confused mass, in which broken cells, granules, oil globules, hair, and epithelium can be detected, presenting a great contrast to the regular arrangement seen in molluscum. In the cases I am describing, the disease appears to have two modes of termination: either the secretion escapes before it becomes hard, and the tumour consequently disappears; or it is retained, and, becoming indurated, gives rise to a permanent wart, which may remain for an indefinite period. I have not witnessed the "curdy suppuration" spoken of by Bateman. The disease was first described under the title of "*molluscum contagiosum*" by Bateman, who separated it from another form not considered contagious, which he named "*molluscum pendulum*." Of the latter, no mention is made by succeeding authors—it probably was a variety of follicular tumour;—but many cases of the former are on record, all closely corresponding with those I have described. Much difference of opinion, however, exists as to the claim it has to its specific name, its contagious nature having been denied by Mr Erasmus Wilson and other recent authors.

It is impossible to resist the conclusion, that the cases originally described by Bateman and Willan, and some by Dr Henderson and Dr Paterson in the 56th volume of the Edinburgh Medical and Surgical Journal, and by Dr John Thomson, as quoted by Mr Wilson in his work on Diseases of the Skin, were propagated by contagion; although some, which occurred to Dr Carswell, and recorded by Cazenave, and others mentioned by Mr Wilson, would equally well show its non-contagious nature.

All authors are agreed as to the seat of the disease, referring it to the sebaceous glands; but a difference exists as to its pathology, some believing it to consist in the development of a peculiar organism, capable of propagation by contact, under favourable circumstances; others regarding it merely as an inspissation of natural sebaceous matter, from obstruction of the duct. It is not impossible to reconcile these opposite views, by showing that both may be correct. A simple obstruction of the orifice of a duct, from any accidental cause, as a primary condition, must occasion an imprisonment and consequent inspissation of its contents, and produce a tumour not differing from one caused by the production of a morbid secretion unable to escape, except in the single circumstance, that the latter might become extended by contagion. Such I believe to be the true nature of the disease, shown by an analysis of the cases already recorded, and confirmed by those I have described.

The consistence of the secretion appears to have differed—in my own cases and those of Mr Wilson, having been approaching to a solid, but in those of Dr Paterson, of a semifluid and milky nature—a difference which would materially alter its liability to contagion, even admitting its composition to be essentially the same; but there is no doubt that both retained sebaceous matter, and a morbid secretion in its stead, have given rise to an affection of the same ordinary characters. The short report I have given offers some points of interest in the history of the disease. The probability of its having attacked the mother when an infant—its reappearance upon herself and three daughters, whilst the father and three other children, two of whom were boys, besides some young neighbours constantly associating with those affected, have all escaped—would lead to the conclusion that it depended upon some hereditary peculiarity derived from the mother, and was not propagated by contagion. The secretion bears a great resemblance to some forms of malignant disease; the history of the cases, however, does not admit of its classification with them; but its cellular structure authorises it to be considered a peculiar organic product, in all probability capable of extension by contagion, under favourable circumstances, which did not exist in these cases, in the nearly solid state of the secretion, and the consequent rarity of its escape. Inoculation has been performed by Dr Henderson, but without effect; and I have not repeated it, believing that as the disease is seated in the sebaceous glands, it must be absorbed by them, to obtain a fair result, which could only be effected by friction over a large surface, which, of course, is not practicable.

As the general health of the patients was remarkably good, the treatment merely consisted in removing the tumours, and checking the tendency to their production by frequent friction of the body with a rough towel, and ablution. In their early stage the application of nitrate of silver never failed to effect a permanent cure; but for their removal when in an advanced stage, I have employed three modes of treatment, viz. removal by scissors, ligature, and pressing out the secretion through a small puncture, followed by the use of nitrate of silver. The last of these has succeeded best. By the use of scissors continued bleeding has occurred, the ligature is only applicable where the tumours are contracted at their bases; and on one occasion severe hemorrhage followed its use, by the tumour being pulled off by the little patient; but where the last treatment was applied, healing soon followed, without a return of the disease, or the appearance of a scar.

The conclusions at which I arrive are the following.

First, The disease hitherto called *molluscum contagiosum* may or may not be contagious, according to the character of the secretion which gives rise to it; which in some cases is simply indurated sebaceous matter, and in others an organized product of a different nature.

Second, That it may arise from some unknown peculiarity of organization, which may be hereditary.

Third, That whatever may be its origin, it is an affection which rarely affects the general health, and is easily cured.

4 Bolton Street, Piccadily,
October 1847.

Art. VII.—*Cases illustrating the Nature and Character of Inflammation and Suppuration of the Heart.* By David Craigie, M.D., Fellow of the Royal College of Physicians, &c.

Though a disease under the name of *Carditis* is mentioned in the principal nosological arrangements, as those of Sauvages, Vogel, Sagar, Cullen, Young, and Mason Good, and is more or less pointedly mentioned by writers either monographical or systematic; yet, to accurate observers, who require the evidence of pathological anatomy, the descriptions given, and the statements made regarding this lesion, appeared invested with doubt and uncertainty. The testimony of Baillie shows that the disease is rare, and is almost never primary, but the result of inflammation of the pericardium, from which it spreads to the filamentous tissue, and partly to the muscular fibres of the heart. Corvisart, indeed, published, under this name, various cases, which, however, have since been admitted to be examples of *pericarditis.* In 1808, Dr John Ford Davis published an interesting small volume, containing an "Inquiry into the Symptoms and Treatment of *Carditis*, Illustrated by Cases and Dissections." In this treatise the author gave three cases, in all of which the pericardium was inflamed, lined by lymph, and adhering to the cardiac pericardium. In the first, which took place in a boy of 7 years, the author states that the muscular substance of the heart was not diseased. In the second, which took place in a young lady of 16, there was distinct *endocarditis* of the right ventricle, and, in a slighter degree, in that of the left ventricle, but no distinct change in the muscular substance of the heart. In the third alone, which occurred in a young lady of 12 years, with the usual marks of inflammation and adhesion of the pericardium, it is stated that the muscular substance of the heart was inflamed to some depth, while the heart appeared enlarged, and was pale and flaccid internally.*

These cases were in general allowed to be examples, not of carditis, but of pericarditis; and, apparently, the author himself took latterly this view; for, in a second inquiry respecting pericarditis, or rheumatism of the heart, in which he gives four cases, he regards these as instances of *carditis* or *pericarditis.* Since the appearance of the first inquiry of Dr Davis, indeed, Mr Dundas, Dr Wells, and Dr Pitcairn had all published on *pericarditis* in connection with rheumatism, and this circumstance had tended to render the anatomical distinctions between *carditis* and *pericarditis* more accurate.*

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* An Inquiry into the Symptoms and Treatment of Carditis, on the Inflammation of the Heart; illustrated by Cases and Dissections. By John Ford Davis, M. D., Member of the Royal College of Physicians, London. 12mo. Bath, 1801.

Facts and reasons of the kind now mentioned induced Laennec to confirm the statement of Baillie, and to maintain, that though partial inflammation of the substance of the heart, in minute spots, is not uncommon, yet, general inflammation of the cardiac substance, either acute or chronic, is a circumstance almost unknown in the records of medicine. The possibility of the fact he nevertheless admits on the evidence of the case recorded by the elder Meckel.

Many years ago it was pointed out in this Journal, that Laennec might have found still more decisive proof in the facts of a case given by Mr Stanley, who is almost the first who describes this lesion with accuracy. Since that case was published, in 1816, several other instances of the lesion have occurred; and one has been published, in 1846, by Mr Stallard of Leicester; and it may be not improper to give a short abstract of the most important of these cases.

1. The case given by Mr Stanley is to the following effect:—

A boy of Christ's Hospital, aged 12, though of delicate frame, yet had generally enjoyed good health, and on Saturday, the 20th April 1816, he seemed to all appearance quite well. Next morning, however, he was brought to the infirmary of the institution, presenting at that time the usual symptoms of fever, namely, great heat of surface, quick pulse, and a white furred tongue. Next day, Monday, the febrile symptoms were aggravated; but the only pain of which he complained was in the left thigh and knee; and this subsided before night. In the afternoon he became delirious, with much watchfulness. On Tuesday, the delirium was very considerable, but without any tendency to coma, and the pupil of the eye was dilated, but not insensible to light. He complained but little of pain; but, when interrogated, he pointed to his forehead. Early in the afternoon of the same day, a convulsive fit took place, but soon went off. In the evening, all the symptoms were aggravated, and the night was passed almost without sleep.

On the following morning he appeared much sunk, and the breathing for the first time became difficult. He was then sufficiently sensible to answer any questions put to him, but soon after he became insensible, and gradually declined, till about two in the afternoon, when he expired.

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* A Second Inquiry respecting Pericarditis, or Rheumatism of the Heart; with Cases. By John Ford Davis, M. D., &c. Bath, 1832.

At no period of his illness did he complain of pain in any part of the chest, nor was there any irregularity, either in the action of the heart or pulsations of the arteries.

The treatment, until Tuesday morning, had been directed to counteract general fever, namely, evacuants and antimonials, with the warm bath. Leeches were afterwards applied to the temples, a blister to the nape of the neck, and a purgative of calomel and rhubarb was administered.

Inspection.—As the symptoms appeared to denote that the cause of death was seated in the head, this part was first examined. But, after attentive examination of the brain, nothing further could be remarked than that the vessels were generally turgid, yet not more so than is frequently seen when death has taken place under circumstances that lead to no suspicion of affection of the head. One circumstance alone is noticed; that though the boy was 12 years of age, yet the thickness of the skull far exceeded what is usual in an adult. There was no appearance of *diploe*, the bone being uniformly solid and dense throughout, and hence the skull-cap was exceedingly weighty. The sutures were almost universally obliterated, scarcely the least vestige of them being perceptible in either table of the skull.

The lungs were healthy.

The pericardium contained four or five ounces of turbid serous fluid, with flakes of coagulable lymph floating in it. The internal surface of the membrane, both where it constituted the exterior bag (capsular *pericardium*), and the reflected layer upon the heart (cardiac *pericardium*) was lined in various situations with a layer of lymph of a reticulated appearance. The size of the heart was natural in relation to the age of the individual. On dividing its parietes, the fibres were exceedingly dark coloured, almost black. This evidently depended on the vessels being loaded with venous blood. The fibres were very soft, and torn in their texture, being easily separable, and with facility compressed between the fingers. Upon looking closely to the cut surface exposed in the section of either ventricle, numerous small collections of dark-coloured purulent matter were visible in distinct situations between the muscular fasciculi. Some of these depositions were situated deeply, near to the cavity of the ventricle, while others were more superficial, and had elevated the cardiac pericardium from the heart. The muscular fibres of the auricles were also softened in their texture and loaded with blood, but without any collections of purulent matter between them. All the cavities of the heart were loaded with coagulated blood. The internal lining valves, and every other part of the organ, presented nothing worthy of remark except the state of general turgescence in the capillary vessels, which had also extended to the lower part of the *trachea* and *bronchi.*[*]

It cannot be denied, that, in this case, the muscular tissue of the heart presented marks of inflammation. It might, indeed, be said that the purulent deposits were seated in the intermuscular filamentous tissue, which, though delicate, is abundant. The case, nevertheless, is a decided instance of inflammation affecting the muscular fibres, which were thereby rendered soft and lacerable.

Mr Stanley himself admits the distinction between abscess in a distinct cyst, and suppuration generally affecting the heart. He states in the same paper that he had not long before met with an instance of the former lesion, where a considerable abscess had been formed in the parietes of the left ventricle, near the apex, while, opposite to the situation of the matter, there was ossification in the lining of the ventricle, with surrounding ulceration. He adds, that he had not been able to meet recorded any instance in which, as an immediate effect of acute inflammation, suppuration, had extended generally throughout the muscular structure of the organ.

The truth is, that abscess of the heart is a rare affection, and suppuration is still rarer. In general, the former is more chronic in its progress than the latter, which is usually acute.

2. In 1828, Dr P. M. Latham, after remarking the rarity of finding purulent matter as a proof or effect of inflammation of the substance of the heart, added, that, nevertheless, he had met with two examples of this lesion. In one instance, the whole heart was deeply tinged with dark-coloured blood, and its substance was softened; and, upon section of both ventricles, innumerable small drops of purulent matter oozed from various parts among the muscular fibres. This was the result of a most rapid and acute inflammation, which terminated in death, after an illness of only two days' duration.

3. In another instance, after death, which terminated an illness of long duration, and characterized by symptoms referable to the heart, a distinct abscess was found in the substance of the left ventricle, closed externally by a portion of adherent pericardium, and connected internally with an ossified portion of the lining membrane.[†]

4. In the descriptive catalogue of Mr Langstaff's museum are several preparations in which the muscular substance of the heart

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[*] Case of Inflammation of the Muscular Structure of the Heart. By Edward Stanley, Esq. Medico-Chirurgical Transactions, vol. vii. p 323. London, 1816.

[†] Pathological Essays on some Diseases of the Heart; being the substance of Lectures delivered before the College of Physicians. By P. Mere Latham, M. D., Physician to St Bartholomew's Hospital. London Medical Gazette, vol iii. p. 119. London, 1829. Essay III.

is said to have been inflamed. No. 298, a female of 46 ; 303, a boy of 8. (The muscular structure showed all the signs of *carditis.*) 308, a man of 47. No specific details, however, are furnished.* In the Morbid Anatomy Collection of the University of Edinburgh, is a preparation of a heart (p. 12), in which a large abscess, with an irregular inner surface, which contained lymph and purulent matter, occupies the whole extent of the *septum cordis*, and communicates, by a small orifice the size of a quill, with the left ventricle. In this case, inflammation must have affected the *septum*, perhaps the neighbouring parts of the left ventricle, and may have eventually been concentrated on the *septum*, in which it induced suppuration, confined within the walls of a distinct cavity. Twelve months previous to death, the patient laboured under symptoms of pneumonia, and at last died suddenly.

5. An instance not less decisive is recorded by Mr Salter of Poole, in the person of a man of 50 years. The disease appears in this case, however, to have been at first slow in progress, and chronic in duration, as the symptoms continued for about six weeks.

N. P. S., about fifty years of age, tall and well-formed, of fair complexion, by trade a glover, but formerly a dragoon, applied, on the 16th March 1834, for advice with the following symptoms. He complained of uneasy sensations in the region of the stomach and under the sternum, increased by any sort of exertion. The appetite was good, the bowels regular, the tongue clean at edges, but the centre and back parts covered with a thin white fur. The pulse was natural ; the face pale ; the pain was not affected by taking food. On applying the ear to the cardiac region nothing abnormal was observed.

These symptoms were first observed about six weeks previously, while walking. The pain was then at the lower part of the chest, inclining to the left side ; and though it did not continue long, was very severe, causing faintness and cold perspiration. About a week subsequently he had another severe paroxysm, which took place immediately after a walk of three miles into the country. The attacks were becoming more frequent, though less severe than at first. They now occurred when sitting ; and even lifting the arm sometimes brought on the pain. With the pain there was considerable uneasiness about the middle of the left upper arm. There was no cough nor sign of any pulmonary disorder. He complained of flatulence.

He was ordered pills containing blue pill and extract of colocynth to be taken every evening ; and to have daily a mixture consisting of sulphate of magnesia and carbonate of soda. From the use of these remedies, and attention to regimen, he experienced relief.

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* Catalogue of the Preparations illustrative of Normal, Abnormal, and Morbid Structure, constituting the Anatomical Museum.

Early on the morning of Saturday the 22d, however, Mr Salter was hastily called to see the patient. He was then sitting up in bed, being, from great oppression and distress in breathing, unable to lie down. He placed his hand over the sternum, the seat of his suffering. The pain was of a dull heavy character, aggraravated by deep inspiration. The action of the heart was natural; pulse 80, regular; the chest sounded well on percussion, and the respiratory murmur was good. The face, however, was pale, the features sharp, and the expression anxious; and he said he felt that he could not live. Blood was drawn to 10 ounces, and a blister was applied over the sternum.

In the evening the symptoms were not abated, the seat and character of the pain remaining unchanged. The pulse had risen to 120, and was small and weak. The respiratory murmur was heard on both sides of the chest, but the anguish of the patient rendered it difficult to make any satisfactory local examination. His sufferings and distress in breathing were at this time so great, that he appeared unable to give any intelligible account of his feelings. One grain of opium and four grains of calomel were directed to be taken every three hours.

On the morning of the 23d (Sunday), when Mr Salter was again called to him, he was found to have spent a restless agitated night. He had vomited much fluid tinged with green bile. He was still obliged to observe the erect position. Sounds of the heart only distinguishable by the stethoscope; the pulse was very feeble, 130 a minute. The pain under the sternum less complained of; but the anxiety and oppression in breathing are most distressing.

On the 24th, after a sleepless night, he became gradually weaker, with cold extremities, and died at 11 at night, about 65 hours after the accession of the acute symptoms. The mind was undisturbed during the whole period of his illness.

Inspection presented the following phenomena. The corpse exhibited no appearances of emaciation. The front of the chest sounded on percussion well at every part.

The sternum being raised, the right lung collapsed, and had a natural aspect; but the opposing pleura on the opposite side being almost universally adherent from former attacks of inflammation, the left lung did not collapse. In the right pleural cavity there was more than half a pint of serous fluid. Nearly the whole of the left lung was in a state of considerable engorgement, resembling the first stage of pneumonia; it was crepitant, but from the cut surfaces escaped a large quantity of sero-sanguineous fluid and from one of the cut bronchial tubes a clear fluid was observed to flow, which by pressure was also forced out of the trachea.

The capillary and other vessels on the bag of the pericardium were distended with red blood, especially on the left side, exhibiting a beautiful vascularity. That portion of the membrane investing the substance of the heart showed also, by its unusual vascularity, striking evidence of having suffered from inflammation; this, too, was mostly on the left side, that portion covering the left ventricle being its chief seat; but the part of the pericardium which had sustained the most intense vascular disturbance was that which lies on, and is attached to, the diaphragm. Here there were not only a distended state of the capillary and larger vessels, but numerous ecchymosed spots and blotches resembling those observed on the skin in purpura hæmorrhagica. There was no lymphy, serous, or purulent effusion into the pericardial sac.

The heart was somewhat larger than natural, its substance of moderate firmness; large coagula were found in all the cavities; some of these were quite white, others deprived of their red colour, and of a yellowish hue, and adhering with great tenacity to the internal parietes, whilst some portions of the coagula were dark on the outside and bleached within. There were no signs of disease in the lining membrane of the heart or valves. In the ascending aorta there were the appearances of commencing ossification. The great deviation from the normal condition of the heart was, however, found to be in the muscular substance of the left ventricle. Excepting a small portion of a few lines in thickness on either surface, the left ventricle had entirely lost its muscular colour; it was of a lightish yellow hue, but still preserving the fibrous character of muscle. From all the cut surfaces of the various sections which were made could be scraped purulent matter. In some parts absorption had taken place, leaving small cavities in the muscular substance, varying from the size of a pin's head to that of a small pea; these were all filled with pus.

The stomach was large and flaccid, and contained a considerable quantity of fluid. The mucous membrane of the pyloric half was of a dingy red colour, from a spotted vascularity. The liver appeared quite healthy. The jejunum was inflamed, both in its peritoneal and mucous coats, for the greater portion of its length.

There were no other diseased appearances.*

Besides this species of diffuse suppuration, inflammation of the substance of the heart causes a species of softening of the muscular fibres, sometimes of a dull pale tint, in other instances of a brownish colour. The former, it is probable, is one of the forms of fatty infiltration. Of this an instance is given by Corvisart, which is afterwards quoted by Bouillaud as an example of inflammation of the cardiac substance.

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* Case of Carditis, by Thomas Salter, Esq., F. L. S. of Poole. Medico-Chirurgical Transactions, Vol. xxii. p. 73. London, 1839.

6. A currier, aged 67, who had for thirty years suffered under some constraint in respiration, was attacked with pain in the left side, and increased difficulty of breathing. Two days after he coughed up some blood; and death took place in the seventh day of the disease.

The heart, natural in volume, was pale, soft, and flaccid. The fleshy fibres of the ventricles and auricles were pale and yellowish, and looked as if a quantity of fatty matter had been deposited between the fleshy fibres, which appeared separated from each other. At the surface of these whitish walls, and in the interior of their fleshy substance, was a vascular net-work well developed. The fleshy substance, when pressed slightly between the fingers, was easily reduced into a sort of pale-coloured pulp.

7. In another case given by Bouillaud, in a young man of 27, who had been for some time ill with catarrh, and afterwards appeared to labour under debility, with great emaciation and dropsy of the lower extremities, death took place in the course of one month after. Besides inflammatory adhesion of the pericardium as thick as three lines, it was found that the heart was small; that its external surface was deep red, interspersed with blackish points; and that its fleshy substance was brain softened and easily torn.

8. M. Raikem gives the following case. A female servant, while working in moist cellars, and sleeping in newly-finished buildings, was attacked in her thirteenth year by acute gouty rheumatism. Of this she partly recovered, but relapsed at the age of 16, and, in consequence, the fingers and toes acquired a morbid and unnatural contraction, not admitting of extension. In July 1808, when she was 36 years of age, she had a third attack of articular rheumatism, which affected all the joints of the extremities, and terminated at the end of fifteen days, leaving the lower extremities œdematous.

On the 5th November of the same year, she was attacked with shiverings, followed by irregular and tumultuous motions of the heart, lancinating pain in the precordial region, breathlessness, and fainting, which lasted one hour. These symptoms increased; the fainting becoming more frequent; and death took place fifteen days afterwards, on the 20th November.

The lower lobe of both lungs was solidified.

The heart was double the natural size, but soft and flaccid. The left ventricle was increased in size, and its walls were attenuated. The mitral valve was thickened and cartilaginous. The auriculo-ventricular opening was so much contracted, that its diameter was not more than six lines.

At the posterior and superior surface of the left auricle, while the pericardium was sound, the muscular texture of the heart was covered with small whitish points, injected with a blackish-red colour, and presented three or four spheroidal tumours, from two to three lines in diameter, raising the pericardium, and furnishing, when divided, true sanious opake purulent matter. The auricular appendix of the same side was hard, firm, large, and contained sanious purulent matter.*

In these cases, the inflammatory affection of the heart was connected with a previous or concomitant attack of articular rheumatism. The following is another example of the same combination.

9. A surgeon aged 58, subject to rheumatic disorders, had a severe attack, with cold extremities, general debility, hard contracted pulse, strong tumultuous action of the heart, and oppressed breathing. Blood-letting was employed, without good effect. The patient died some hours after in a fainting-fit.

Beneath the adipose layer of the heart were half a dozen small purulent abscesses. The *endocardium* was in various parts red. Through this membrane were seen, elevating themselves on the pillar of the mitral valve, two yellowish-white points, which on incision poured forth purulent matter. One of these abscesses was in the substance of the pillar. A thin exudation covered the inflamed portions of the membrane. In different points of the substance of the *septum* were observed abscesses two or three lines in diameter, all filled with yellowish-white purulent matter. The portions of cardiac substance void of abscesses were yellowish-gray, softened, and were torn by the slightest effort.†

10. In the case of a young soldier of 19, who was admitted to the hospital of Gros-Caillou, on the fourth day of an attack of small-pox, abscesses, eschars, and dropsy of the left upper extremity, in consequence of *phlebitis*, ensued in the course of the disease. The patient did not recover, but became greatly emaciated, and died on the fifty-fifth day.

At the base of the left ventricle, behind the mitral valve, in the substance of the fleshy part of the heart, was an abscess as large as a walnut, containing white purulent matter, well formed, homogeneous, not in any degree caseous, without any communication with the exterior or the interior of the heart, enclosed within a firm cyst. The heart presented no other trace of suppuration.

It is possible that this last instance may have been the effect of absorption of purulent matter by the veins. At all events it is a good example of encysted abscess of the heart.

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* Extrait d'une Observation sur une Cardite Aigue, &c. Par M. Raikem Bulletins de la Faculté pour l'année, 1809.

† Dissertation Inaugurale sur la cardite partielle et generale. Par M. Simonet.

11. The following case, which is recorded by M. Buisson, shows that mere softening is an effect of inflammation. A female, aged 53 years, entered the Hotel Dieu, at Nismes, on the 16th of May 1834, after having been eight days ill with acute pain beneath the right breast. The face was pale; the cheeks coloured; and the patient was anxious and breathless, with frequent cough, and reddish-coloured expectoration, void of froth. Part of her uneasiness she ascribed to shocks by a carriage.

Respiration was normal on the left side; on the right it was obscure from above anteriorly and superiorly, and extinct behind.

The impulse of the heart was very feeble.

The treatment consisted of a small general blood-letting (eight ounces); a cough mixture; leeches on the painful part, antimonial solution, a blister on the side, and sinapisms to the arms. Death took place about one in the morning of the second day. Inspection twenty-four hours afterwards disclosed the following appearances.

The left lung was sound. The right lung was crepitating in some parts; its lower lobe was infiltrated with purulent matter; and the posterior part of the other lobes was in a state of red hepatization and softening. The pleura was lined with lymph of some thickness; and its cavity contained one pint of seropurulent fluid.

The heart was twice the volume of the closed hand of the subject. Its muscular substance was very much softened, and was easily torn; its colour was deep red; and the injection was considerable. Several ounces of serum were contained within the pericardium; and the cardiac pericardium was reddened and covered by a coating of lymph. The *endocardium* of the left ventricle and the inner membrane of the aorta were tinged of a deep red.*

12. In an instance of partial inflammation of the substance of the heart, described by M. Gintrac, purulent matter was formed in the *parietes* of the left ventricle, and being near the surface, it burst into the pericardium.

13. It has been seen how obscure sometimes are the symptoms attendant on the presence and formation of abscess of the heart or suppuration taking place in its substance. The following case by Dr Richard Chambers, resembles in several points of its symptomatology the case given by Mr Stanley. A boy aged 14 was admitted, on the 6th June 1844, into the Essex and Colchester Hospital, under the care of Mr Partridges, with disease of the ankle-joint. He continued gradually to improve to the morning of the 20th June, when he was seized with epileptiform convul-

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* Fragmens pour service a l'histoire des alterations organiques du coeur. Par F. Buisson. Theses Montpelier, No. 48.

sions. At 10 A. M. Dr Chambers found him semicomatose, with dilated pupils, distended jugular veins, weak pulse, scanty urine, and the bowels confined. After the application of eight leeches behind the ears, and the administration of a turpentine enema, and a saline diuretic, consciousness and apparent recovery took place in the course of a few hours. During the night, however, he relapsed, and died on the following day.

On inspection thirty-six hours after death, the pericardium was found thickened and strongly adherent to the heart, and an abscess containing about two ounces of matter was found extending from auricle to auricle round the apex of the heart, and lodged in the substance of the organ.*

14. In the following case by Mr Chance the chief symptoms were those of affection of the brain.

A lively but delicate lad, aged thirteen, of a scrofulous diathesis, was apparently in perfect health on Sunday, December 21st, 1845, and on that day partook of a hearty dinner of roast pork. In the afternoon he complained of pain in the stomach and head, but this was considered trifling, and he was sent to walk. In an hour he returned to tea, and he again made a hearty meal. After being in bed a short time he felt sick, but no vomiting occurred until two o'clock in the morning, when the food returned unchanged. In the morning he appeared in a drowsy, half-conscious state, disinclined to move or speak. At eleven o'clock Mr Ray, the surgeon in attendance, described him as being in a low state, disinclined to speak, pale, and faint; with rapid and indistinct pulse; tongue thickly coated in the middle by a dry brown mark about as wide as the finger, on each side it was white, whilst its edges were red. He complained of sickness and pain in the stomach, and he looked like one who had been sick. Immediately after Mr Ray left, he became more and more insensible, so that at two o'clock P. M., he was in a state of perfect coma, with contracted pupils, the mouth firmly contracted, the pulse too rapid and fluttering to be counted, and with occasional convulsive movements of the arms and legs. On Tuesday he was in the same condition, but more convulsed, and on the cheek a distinct pustular eruption had made its appearance, the pustules as full and distinct as small-pox, occupying the space of half-a-crown, and having an inflamed base; besides these he also had a few on his hands. He died at three P. M.

Inspection twenty-four hours after death furnished the following results.

Upon removing the calvarium the veins and sinuses of the *dura mater* were found slightly distended with blood; the vessels upon the convolutions of the brain were more congested than usual; no inflammation observed anywhere; lungs healthy. The pericardium contained about half an ounce of fluid, like pus and water, or, rather, like the contents of the stomach. The vessels of the heart were of large size, and the heart itself was studded all over with black and red spots of effused blood, varying in size from a mere point to a third of an inch in diameter: these spots were most numerous upon the left ventricle, and especially at its upper and back part. The heart was apparently in a state of violent contraction; upon the anterior surface there was a highly inflamed spot, as large as a shilling, covered with coagulated lymph, having a rough aspect; the surface beneath presented the same elevated appearance as abscess in other parts of the body; the texture of the ventricle was firm and dark-coloured, except near the inflamed part, where it was a shade lighter. Upon cutting across this spot transversely, a slit was discovered communicating with an abscess, which was about half an inch in diameter, and this had an opening in the centre of the patch of lymph, through which a probe could scarcely pass; but in the opposite direction the probe passed freely into the cavity of the ventricle.*

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* On Valvular Disease of the Heart, &c. By Richard Chambers, M. D., Colchester. Lancet, 27, 1844. P. 557.

15. The following case is by Mr Stallard.

"John Perry, aged sixty, shoemaker, of a short, thick-set, though bloated frame, an inmate in the Leicester Union Workhouse, was seized, whilst at work, with coma, cyanosis, and great prostration. As I happened to be near at hand, I was summoned immediately. I found him with a cold clammy skin, the whole surface of a blue colour, and the face suffused with perspiration. He lay in a semicomatose condition, but was sensible when aroused; the pupils acted naturally; the pulse full, soft, and feeble, beating 60 in the minute; the respiration slow and gentle. I administered some hot brandy and water, and ordered it to be repeated twice in the day; he also took a dose of common aperient medicine, and his food was to be light and nutritious.

March 23d, 1844, being the following day. He reported himself better, and stated that he had felt quite well up to the period of his seizure, and that he had never been subject to similar attacks. The pulse is to-day stronger and more frequent, but the respiration is still slow, though not so slow as yesterday; the skin is warmer, and has lost much of its blueness; the heart was carefully examined, its sounds were feeble, but not abnormal in any other way. He has had frequent threatenings of faintness, which have been relieved by brandy and water.

He remained in this state two days, gradually becoming more feeble, and Monday, the 25th, I went as usual to see him, and to the surprise of the nurse, who supposed him to be asleep, I found him dead.

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* Case by Mr Chance, Lancet, May 1846.

Post-mortem examination.—The lips, abdomen, and extremities were of a dirty blue colour, as though incipient decomposition had already commenced. Head not examined.

The cavities of both pleuræ contained much dark-coloured serous effusion; the lungs were congested with black blood, though crepitant.

The heart was fat, flabby, and rather larger than usual, and the pericardium contained about one ounce of dirty serum; the right auricle and ventricle were, comparatively with the left, of normal size, and the valves were quite healthy; the lining membrane was of a deep port-wine colour. On opening the left ventricle, which was accomplished by a V shaped incision, an abscess was observed. It was situated at the apex of the ventricle, and of a very irregular shape, being most pointed towards the apex of the heart, from the surface of which it was separated by two or three lines of healthy structure; above it projected considerably into the cavity of the ventricle, with which it communicated by a small fissure; the interposed septum was about one line thick, and appeared to consist of thickened endocardium. The cavity of the abscess contained bloody purulent-looking fluid; its lining membrane was of a light red tint, and presented a granular appearance. Around the abscess the muscular tissue was darker than natural; and in the outer wall of the same ventricle were observed several fissures containing a dark-coloured fibrinous material, and some of these, though not all, communicated with the cavity of the ventricle. The coronary arteries were much ossified.

The other organs presented the appearances of poisoned and decomposed blood.

The liver was soft and congested; the intestines presented the deep dirty-red colour observed in the heart. The kidneys were of normal size; the capsule, owing to incipient decomposition, was everywhere easily separated from the parenchyma; in the substance of both were many serous cysts, all of which were lined with a dark-coloured coating of decomposed blood, and the contained serum was also discoloured. The external surface was granulated, but this appearance did not extend more than a line into the cortical structure. The whole body rapidly decomposed."*

It appears that in some of these cases suppuration of the heart was combined with rheumatism, and that in others it was not.

16. In the following case, which resembles in several points that given by M. Raikem, the affection of the heart was preceded or accompanied by a rheumatic disorder of the right knee-joint.

I was requested, on the 22d April 1843, by a professional friend, to visit a widowed female, aged 35, residing in William Street. I found the patient living in a sort of cellar or sunk apartment, about four or five feet below the level of the ground. She had been ill for six or seven weeks with a rheumatic affection of the right knee-joint, which was then hot, diffusely swelled so as to obliterate the usual figure of the knee-pan, a little painful, semibent, and could not be extended. With this complaint she was mostly confined to bed, but moved occasionally through the house in a halting manner. After examining the knee-joint, it appeared to me that something else was wrong. She had been a little unmanageable, and did not take medicine or do anything regularly, as she was requested to do. The pulse was 108—110, rather strong; there was strong action of the heart, though no irregularity in rhythm could be observed; and a little blowing in first beat, which was prolonged, was audible. She made, however, no complaint of pain in the region of the heart or difficulty of breathing. Percussion elicited natural sounds. A blowing sound was also audible in both carotid arteries, which pulsated rather strongly.

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* Observations on the Pathology of Abscess of the Heart, with a case. By J. H. Stallard, Esq. T ansactions of Medical and Provincial Association, Vol. xv. London, 1847.

The blowing sound in the cardiac and arterial beat I was disposed to ascribe to some degree of nervousness and alarm at being visited by a stranger. At the same time the aspect and manner of the patient were so peculiar, that I was quite satisfied there was something more the matter than the affection of the knee-joint. The eyes were glaring, and the cheeks slightly flushed. The manner of the patient was hasty and wayward; and her answers were given in a way which showed that she did not tell the whole truth of her disorder. The reason of this will appear in the sequel.

This woman, I have said, was living in one of two apartments, partly underground, and the house was manifestly damp and probably cold. It appeared to me, that of this the rheumatic disorder was one of the results; and as it seemed impossible to expect recovery, whatever treatment might be pursued, I suggested to the gentleman first, that she should be removed to the Infirmary. For this my friend was most anxious. But the patient herself instantly expressed the most violent aversion, and became angry and quite unreasonable on hearing of the proposal. Part of the truth then came out. She spoke in a wayward and rather incoherent manner; and I was satisfied that she was about to become deranged. After patient reasoning with her a little, and pointing out the inconveniences of her present abode, and the advantages she might enjoy in the Infirmary, with the solicitation of one of her relatives, she at length gave a sort of partial consent, and the same afternoon she was removed thither.

The first thing done was to procure sleep, which had, from various causes, not of late been very regular or refreshing; and next to that, to get the bowels moved. The pulse was still above 100, strong and tense. The knee continued in the same state—semi-bent, swelled, hot, and incapable of any exertion, much pain being produced by any attempt at extending the leg or the thigh. In the course of examining the chest and abdomen, I found considerable swelling in the hypogastric region, and saw at once that the patient was pregnant. The beat of the fœtal heart and the placental murmur were distinctly heard. I accordingly, to avoid hurting her feelings, asked the question, if she had no reason to believe herself in that condition. She instantly became quite angry and almost furious, asking me how she could be with child when her husband was dead. I waited till this tempest of passion was past; and then told her, that I could not at present form any other opinion of the cause of the abdominal swelling than pregnancy; and that though I would say no more on the subject, it would be improper, in conducting her treatment, to overlook the fact.

The fact which I had just ascertained led me to hesitate in adopting, for the removal of the rheumatic affection, any very active treatment by meadow-saffron or other means; and all that was ordered was an antimonial opiate at bed-time, and a liniment to be rubbed gently over the knee, keeping the joint covered with flannel.

Next day she was no better; and in the evening of that same day (25th) she became delirious, and so unmanageable, that the nurse in my absence put on a strait waistcoat. This I ordered immediately to be removed, and the patient became more calm; but she still spoke deliriously, had occasional visions, and the tongue was tremulous. She was also sleepless and restless during the night. Information was now got, that she was aware that she was pregnant; that she had taken medicine with the intention of procuring abortion; and besides this, she had drank spirits to considerable extent daily when at home, with the view of lulling her apprehensions.

Amidst this conflict of feelings, she became daily worse. Delirium continued, during the fits of which she spoke often in reference to the possibility of her being pregnant; confessing the fact at one time, and denying it at another.

She had also fits of difficult breathing, amounting to *orthopnœa*, during which she required to be supported in the erect position, from the dread of suffocation. When these fits subsided, she could lie on the back or either side, and was occasionally found so lying without any urgent complaint. The cardiac impulse continued strong, with blowing murmur on the first sound, and at the rate of 120 beats in the minute. No rough murmur was heard, but occasionally a jarring sound below the sternum, about two inches above its lower extremity, was audible; and the range of cardiac action was greater than usual. Some dulness was audible over the cardiac region.

The urine varied from twelve to fifteen ounces in twenty-four hours, was very high-coloured, and deposited a copious sediment of uric acid and urate of ammonia.

The symptoms proceeded with little change. Delirium was either constant, or at least so frequent, that the patient could not be depended on for one hour. Yet at times she gave rational and intelligent answers. At all times she looked wild, alarmed, and suspicious; and from the slightest causes, and often without any cause, became alarmed and apprehensive. After several more fits of difficult breathing, she died on the 4th of May, at 7 in the evening.

Inspection of the body on the 6th disclosed the following facts.

The membranes of the brain, and the brain itself, were not more vascular or redder than natural. Some serum was contained in the subarachnoid tissue, over the hemispheres, but little fluid was found in the ventricles.

Excepting old pleuritic adhesions, the lungs were healthy.

The pericardium contained some opake yellow-coloured serous fluid, and the free surface of the capsular and cardiac pericardium was covered by a layer of soft yellowish lymph, thin on the ventricles, but a little thicker at the base of the heart and over the auricles.

The heart was not larger than usual in an adult female. The tricuspid valves, and the semilunar valves of the pulmonary artery were natural. The mitral valve was a little cartilaginous and firm. The aortic valves were much diseased.

At the attached margin of the right semilunar valve was an aperture admitting a probe beneath the membrane into a small cavity. Near the left angle of the valve was another aperture a little larger. The posterior or middle valve was perforated by various irregular holes, and it was so soft as to give way easily. It was covered with reddish soft lymph, especially at its attached margin. On the upper or aortic side of this valve was an aperture and cavity, similar to those behind the right valve. The left semilunar valve presented at its right angle, where it joins the middle valve, some red-coloured lymph, but was elsewhere natural.

It was then found that the middle and right valve, with the right angle of the left valve, were detached from the interior of the aorta by suppuration, and that a cavity, containing purulent matter, was formed in the auricles, and had thence extended to the adjoining part of the aorta. This cavity was divided into two compartments by the septum of the auricles; one compartment being over the right auricle, and the other over the left auricle. Each abscess thus formed in the substance of the auricles formed a convex swelling projecting within the auricles. These cavities, which were about the size of large beans, contained purulent matter mixed with blood. The adjoining portions of the auricles were thickened and infiltrated with bloody serum, and softened and lacerable.

A fœtus of about six months was found within the uterus.

The preparation of this heart is preserved in the museum at the university.

On laying open the right knee-joint, the synovial membrane lining the *patella* and covering the upper articular surface of the *tibia*, and the anterior part of the condyles of the *femur*, was found greatly reddened and vascular. That covering the head of the tibia was quite crimson. The free surface of the synovial membrane was covered in various parts with adherent soft lymph, especially at those parts of the articular surfaces not immediately in contact with each other. No purulent matter was contained within the synovial cavity; but the whole surface was covered with a turbid fluid, which seemed to be synovia mixed with bloody serum. The free surface of the synovial membrane was quite roughened and irregular, partly by the vascular redness, partly by effused lymph and blood globules. The synovial membrane covering the inner head of the tibia, which was particularly crimson-coloured, seemed to be giving way under a process of absorption; for various perforations were perceptible in it. This I cannot call ulceration, but it might have proceeded to that. The semilunar cartilages were apparently softer and thicker than natural, and their synovial membrane was also highly vascular. All these changes were confined chiefly to the anterior part of the articulation. The posterior portion was comparatively healthy.

The deposit over the articular synovial membrane was in certain points gritty.

The synovial membrane, lining the capsule of the joint, seemed thickened. But it was uncertain whether this apparent thickening was not owing rather to lymph deposed over it. It was rough and vascular, though not to the same extent as that covering the cartilages.

The portion of texture forming what is called the capsule of the joint was considerably thickened, and rather tough and firm. On minute examination, this was found to be occasioned mostly by infiltration of lymph and bloody serum in its interstitial tissue, outside the articular portion of the capsule. On making incisions, this effused fluid, which had a gelly-like aspect, oozed away in perceptible quantity.

An account of this case was published in a former number of this Journal, by the gentleman who held the office of prosector at the time.* The present account is from notes taken from the commencement to the termination of the case.

In this case we have an example of synovial or capsular rheumatism of the knee-joint, with inflammation and suppuration of the substance of the auricles of the heart and *pericarditis*. It has been already seen that the union of articular rheumatism with inflammation of the substance of the heart has been previously observed; and the present is another example of this conjunction. It is not easy to say which disease was first, the inflammation of the synovial membrane or that of the heart. They might have been coaetaneous in origin; and that they were so I am inclined, from various circumstances, to think. It is usually said and believed, that rheumatism causes inflammation of the *pericardium* or *endocardium;* but cases occur in which the affection of these membranes is quite as early as the rheumatic disorder in origin, if not previous to its formation. In a considerable proportion of cases, the same state of the system, and perhaps of the blood, may be a cause at the same time of *endocarditis*, *pericarditis*, and rheumatic inflammation of the synovial membranes; and if this be the case, it is easy to see that the same state of the system and blood may induce, along with rheumatic inflammation of any synovial membrane, inflammation and suppuration of the muscular substance of the heart.

In considering this concatenation and connection of morbid processes, it is to be observed that synovial inflammation dependent on rheumatism is often if not always connected with a deficient excretion of uric acid, and probably other nitrogenous principles; and urate of soda is very commonly contained in the lymph effused within the joints in this disease. This shows that an unusual proportion of uric acid, probably with urea, is circulating in the blood, in which it may excite irritation and inflammation in different organs. We know that the presence of urea in undue quantity in the blood is a disposing cause of inflammation of the *pleura*, the *pericardium*, the *peritoneum*, and other membranes; and it is not unreasonable to infer, that the presence of uric acid, which we know abounds in rheumatic disorders, may produce in the *pericardium*, *endocardium*, and even the substance of the heart, the same inflammatory state which it seems to cause in the synovial membranes.

In the case above detailed, inflammation had attacked the substance of the auricles, and proceeding to suppuration, formed the purulent cavity found after death, while the inflammatory process, by extending to the adjoining portion of the aorta, had caused the detachment of one of the valves and the adjoining part of the other two from the cylinder of the artery. I think, nevertheless, that one of these valves, namely, the middle one, had been previously in the cribriform state or perforated by holes; and that this was the main cause of the middle valve being so completely detached from the aorta.

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* Edinburgh Medical and Surgical Journal, Vol. lxvi. p. 276. Edinburgh, 1846.

The complication with *delirium tremens*, though not uncommon, is nevertheless important. It was partly the effect of the intemperance of the patient, partly of moral causes, and partly of the disease already existing in her blood and system. That disease often arises with peculiar facility and rapidity, where there is previously disease of the lungs, of the heart, of the liver, or the kidneys.* In the case now under consideration, not only had the use of spirits contributed to induce that disorder, but the inflammatory state of the system of the patient had doubtless some share.

All the cases now recorded, it must be admitted, afford examples of inflammation either in the muscular substance of the heart, or of the connecting filamentous tissue, or of both together. It is indeed not easy to conceive how the one tissue can be inflamed and not the other; and whether we say that the inflammatory process begins in the filamentous tissue, and thence spreads to the muscular, which in some cases is probable, or begins in the muscular tissue, and spreads to the filamentous, the result is the same.

It is impossible, therefore, to doubt that the muscular substance of the heart is liable to be affected by inflammation, and its usual effects, softening, lacerability of fibres, and infiltration of purulent matter. In one set of cases, indeed, the muscular fibres are softened, and rendered fragile and lacerable; in another, purulent matter is infiltrated between and around them in drops and small abscesses; and in a third, a distinct abscess is formed. From the cases now recorded, it seems proper to infer, that inflammation of the heart is often very acute and rapid in progress, and in other instances is slow, tedious, and chronic. The formation of abscess is in general a slower process than that of general suppuration and purulent infiltration. Inflammation further appears, in some instances, to attack the whole organ; and in other instances, only one part of it; or after attacking the whole, to become concentrated in one part.

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* I must add, nevertheless, that it always appeared to me, after learning the history of this patient, that the diseases which took place in the heart and joints were the effects as much of mental distress, anxiety, and remorse, as of physical causes. The house which she occupied consisted of two apartments, if I remember right—certainly not more than three. One of these she let, and the lodger having become more intimate with her than was right, according to the account of the neighbours subsequently received,—one apartment and one bed served for both. The effect of this was pregnancy. The occupant of the apartment left her after some time, and it was when she looked to the prospect of becoming a mother without any conjugal claim on the father of the child, that she first attempted to procure abortion, then tried to allay anxiety and feelings of shame and remorse, in the excessive use of spirits. The conflict of feelings occasioned by brooding over her condition, appears to have impaired her health and strength, and then made her easily suffer from the bad effects of a cold, damp, underground residence.

In all the acute and rapid cases of the disease, the muscular substance of the heart is of a peculiarly dark-red, brown, or black colour, as well as softened, as if the blood had not been arterialized since the commencement of the process. Blood in drops, broken down blood-globules, and bloody serum, as well as lymph globules and purulent globules, are also extravasated and infiltrated into the interstitial substance of the inflamed part.

As to symptomatic effects and external signs, either the cases are too few and too anomalous to justify the establishment of any positive inferences, or the whole are so confused and contradictory, that they scarcely agree in more than two or three points.

In three of the cases, all young persons, the symptoms indicated so decidedly affection of the brain, that the treatment was chiefly directed to the alleviation and removal of symptoms in that region of the body.

In two of the cases the symptoms were as much those which indicated some affection of the organs of respiration as one of the heart. These symptoms again appeared in paroxysms periodically; and in the intervals the patient suffered in very unequal degrees.

In scarcely one of the cases could it be said that the symptoms observed pointed to the heart alone as the organ principally affected. In the rheumatic cases, indeed, by conjoining the circumstance of the precession or actual presence of rheumatism, a probable conjecture might be formed that the other symptoms arose from affection of some part of the heart.

As far as can hitherto be seen, the natural course of this disease is to terminate in death. We have no evidence to induce us to believe that, when the disease is once established, and has disintegrated part of the muscular tissue, either rendering it soft and lacerable, or causing the infiltration of purulent matter or lymph, or blood-globules, these portions are ever repaired, or life is prolonged. M. Bouillaud nevertheless thinks that inflammation of the heart terminates, in certain cases, not immediately in death, but in the formation of an aneurismal sac in the substance of the walls or septum. This question has to a certain extent occupied my consideration in an article on aneurism of the heart; and it would require me to repeat what I there said, were I again to enter fully into the examination of the subject. At present, however, I think that what is already known of inflammation of the substance of the heart leads to the following conclusion.

It is not impossible that aneurism of the heart may occasionally be one of the effects of inflammation. But it must be inflammation not only very partial, but not acute, completely chronic in short, and such that it merely renders the muscular fibres lacerable at one point, without entirely destroying their organization and continuity. This change may prepare the way for these fibres eventually giving way so slowly and gradually, as to form the aneurismal pouch and recess. Were they to be so much disorganized, as they are after acute chronic inflammation, if the acute disease did not destroy the patient, they would give way so suddenly and completely, that the result would be, not aneurism, but laceration.

The most common way, however, in which I believe aneurism of the heart can be regarded, if ever it is to be so regarded, as the effect of inflammation, is when the endocardium or lining membrane is inflamed. In all the well-authenticated cases of passive aneurism of the heart, the endocardium has been found in various modes diseased; roughened, covered with patches of lymph or bone, and sometimes entirely detached. When inflammation attacks this membrane, it may detach it from the subjacent muscular layer, which is sure to be rendered soft, or brittle, and lacerable. If the endocardium is detached, it is easy for the impulse of the blood to act on the diseased and changed muscular fibres, and thus cause them to give way and form an aneurismal pouch or sac. When this has once taken place, to however small an extent, it will increase daily, until it forms so large a recess that the motions of the heart can no longer be efficiently carried on. The disease then terminates in death.

As to therapeutics, all treatment has been hitherto unavailing, either in consequence of the difficulty of diagnosis, or from the necessary fatality of the malady. It is clear that though the disease is inflammatory, yet large blood-letting seems not of much avail. If the views above given regarding the state of the blood be well founded, it follows that blood-letting, or indeed any remedy which does not wholly alter the constitution of the blood, must be of little utility. If the existence of the disease could be recognized, it seems reasonable to think that a moderate blood-letting, general or local, according to circumstances, would be most appropriate, and next to that, the use of bicarbonate of soda and diluents, or bicarbonate of potassa, in order to counteract the effects of the uric acid, might be beneficial. Nitrate of potassa also, and acetate of ammonia, which have both been found useful in rheumatism, might be advantageously employed, with the view of diminishing the quantity of uric acid, and preventing its hurtful influence. Meadow-saffron, upon the same principle, might be administered; but this should be in conjunction with soda and opium.

PART II.

CRITICAL ANALYSIS.

Art. I.—*Report on the Climate and Principal Diseases of the African Station; compiled from Documents in the office of the Director-General of the Medical Department, and from other sources, in compliance with the Directions of the Right Honourable the Lords Commissioners of the Admiralty. Under the immediate Direction of Sir William Burnett, M. D., K. C. H., F. R. S.* By Alexander Bryson, M. D., Surgeon, R. N. London, 1847. 8vo. Pp. 266.

When it was found, that the cruel treatment, which the Spaniards and other Europeans bestowed on the aboriginal races of America and the islands of the Caribbean Sea, destroyed these helpless people in great numbers, and threatened to exterminate them completely, the Spaniards and Portuguese bethought themselves of conveying to the western Continent and the islands of the Antilles, the black and wooly-haired natives of Africa,—a race more robust, and more tolerant of labour within the tropics. These were found most abundantly along the south-west coast of that large continent, and from that coast, also, they were most easily conveyed;—the run being, on the whole, short and direct to the east coast of America and the adjacent islands. This traffic, which began with the Spaniards, Portuguese, and Genoese, the last of whom commenced, in 1518, on a regular and systematic scale, the importation of negroes from Africa to America, gradually extended among other-European nations, and was in the eighteenth century carried to a very great extent even by Great Britain. Then it was, that it was recognized so generally and fully as a legitimate species of commerce, that the very idea of calling in question its morality or justice was completely ridiculed. Laws and statutes were made to sanction and regulate the mode in which it was to be carried on, and the accommodation which was to be provided for each negro in the voyage from the coast of Africa to the West Indies.

At length, towards the close of the century, various benevolent individuals, themselves persuaded of the injustice and iniquity of buying and selling their fellow creatures, attempted to convince the public and the members of the Legislature, and, above all, those most interested in the trade, that it was one which no Christian nation, if consistent to its principles, could, or ought to tolerate. The society for the abolition of the slave-trade was instituted in London in 1787; and in the course of a few years, by the agency of Mr Wilberforce, a small party, favourable to the abolition of slavery and the emancipation of slaves, was formed in the House of Commons. So slow, however, is the progress of truth, when opposed by prejudice and interest, and especially that which is connected with the love of gain, that thirty years elapsed before any decided measure was taken in putting a stop to an abuse, which ought never to have existed. In March 1807, a bill, embracing the principle to abolish the slave-trade in the British colonies, and among British subjects, and the analogous principle to solicit and obtain the co-operation of foreign powers, was brought into Parliament, and carried through both Houses.

Among other European and American nations, little was done, and that only reluctantly. The United States were the first to follow the example of the English legislature. These were followed by the New South American republics of Venezuela, Chili, and Buenos Ayres; and in Europe, by Sweden and Denmark, Holland and France.

Spain could not be induced to go farther than promise, in 1814, that she would abolish the slave-trade in eight years. Portugal, in 1815, abolished the trade to the north of the equator, and promised final abolition at the end of eight years, on condition of receiving a sum of money as compensation for alleged loss. In neither of these instances did the contracting powers abide by their engagements.

The slave-trade, however, consists of two parts: one the importation of slaves from Africa; the other, the maintenance of these persons in a state of slavery, in the colonies to which they are transported. It was against the first part of the system alone that the new measure was directed. No step was as yet taken to abolish the second part of the system, and emancipate the immense numbers of negroes, mulattoes, and mestizos, in a state of bondage on the continent of America and the adjacent islands. It is one of the evils of slavery, that it is impossible to maintain the numbers of slaves employed in the colonies as labourers, without successive importations. The usual course of nature, by propagation, has been in this instance found to be inadequate to the end; and while the supply required to be kept on a level with the demand, by various means and contrivances, the measure, or law of non-importation, was evaded.

It hence resulted, that the resolution, which, in this country, originated in good and benevolent intentions, though doubtless an important point gained, yet fell far short of accomplishing the good proposed. That measure established certain principles absolutely necessary to the abolition of the slave-trade. It was, however, comparatively inefficient; and what was still more to be lamented, in proposing to do good, it actually was productive of more evil. While the trade was permitted, the subjects of it were treated with care and some degree of kindness; and as the interest of the traders was to convey to the market as many negroes as possible in saleable condition, they endeavoured to avoid all those pieces of cruelty and bad treatment which were calculated to deteriorate the value of the cargo, and diminish the profits to be derived from it. Now, on the other hand, when the trade was prohibited by law, it could be carried on in no other mode than by smuggling, as any other contraband trade; and this tended to the great detriment of the negroes imported. They were not only conveyed in smaller vessels, with less perfect accommodation, but they were crowded together in such numbers, that often they became diseased, and perished in great numbers. In one instance, in a French vessel, violent ophthalmia broke out among a cargo of slaves, destroyed their eyes, and rendered them blind; and then numbers were thrown overboard as useless.

The knowledge that such atrocities were neither few nor unfrequent, led to various decided measures on the part of the British government. A bill was in 1811 introduced into both houses, and carried unanimously, declaring the trade in slaves to be felony, and punishable with fourteen years' transportation, or five years' imprisonment. In 1824, it was made an act of piracy, to be punished capitally; and when, in 1837, the criminal law was revised, and various crimes excepted from capital punishment, this crime was to be visited by transportation for life. After this the practice of importing slaves could no longer be said to be carried on by British subjects. Yet it was believed to be continued to a great extent in the Mauritius.

Inert and inoperative, however, as was the law denouncing importation of slaves, it would have been greatly more so, if Great Britain had not been obliged to take upon herself the invidious and expensive duty of chief guardian of the slave coast. After the general pacification of 1815, accordingly, a squadron, varying from eight to ten or twelve armed vessels, was employed on that station, to seize and capture all vessels having slaves on board. This practice continued for years; and a most harassing service it was. To guard a coast so extensive, and so deleterious to Europeans; to enter rivers and creeks, as was often requisite, in boats; and to pursue to their haunts, under an equinoctial sun, those light vessels employed as slavers, furnished labour and fatigue which very soon told as usual on the health and strength of the most vigorous and enterprising seamen. Large proportions of the crews of every vessel were invariably attacked with the endemic fever of the African coast. Not a few fell victims to its irresistible violence and mortality. Of many the constitutions were shattered beyond recovery; and of all the strength was impaired, and the health in various modes broken. Besides the endemic fever of the coast, which is what nosologists call a remittent or *amphimerina*, agues of bad character attacked the men; and dysentery in certain seasons, and at certain localities, was proved particularly violent and unmanageable.

In numerous instances also was it found, that without the fatigue, exposure, and discomfort of boat service, the crews of certain vessels, on approaching the coast, became affected by fever. The land there, especially near mouths of rivers, is covered with dense fogs and mists, called by the seamen smokes; and often, after being exposed to these smokes for a few days, symptoms of fever began to appear among the ship's company; and one after another was smitten by disease. The only course to be pursued in such cases was to weigh anchor instantly, and run off eighty or ninety miles to sea, or to withdraw to some less deleterious station. Multiplied instances of vessels pursuing this course, and then returning, after losing ten or twelve of their hands, to the station, occurred between 1820 and the present time. Such is the monotonous and inglorious life of the seamen on the African station.

Yet amidst all this evil and fatality, from the climate and coast of South West Africa, the British Government persevered with a degree of firmness that showed they were in earnest. As one set of vessels and crews were withdrawn from the fatal coast, others were sent thither to endure the same labours, to encounter the same dangers, and to fall in the same inglorious manner. It was neither the sword nor the shot of the enemy that did the mischief, and thinned the crews of the squadrons, on the African station. It was a conflict with the physical laws of the globe; it might be said with the laws of nature, and the fixed determination of the Ruler of the Universe. It was an unequal warfare against the elements, and the powers of a climate unrivalled in its fatal influence, against ill-health, disease, and death, issuing from the whole atmosphere, springing up along the whole coast of Africa, from every creek, along every bay, from the banks of every river and from every strand where mangroves grow. Much of this was caused by private adventurers under Spanish and Portuguese colours; and much by the resistance which both these governments made. From 1815 to 1820, and onwards to 1830, Great Britain had to perform this arduous service single-handed. Subsequent to 1830 the subjects of the French Government began to give assistance; in 1833 a treaty was concluded between Great Britain and France, according to which all vessels proved to be engaged in the slave trade, were ordered to be taken up; and, since that time, England has had the co-operation of the French Government, in the duty of guarding the coast and capturing slave-ships.

At length, in 1833, after various difficulties and many impediments, the second part of the measure for abolishing slavery in the British colonies received the sanction of the legislature. This Act, which consisted of two parts, one providing for an intermediate or probationary state, or apprenticeship of six years for plantation negroes, and four years for all others, another granting to proprietors the sum of twenty millions, as compensation for loss of property, received the royal assent on the 28th August 1833. This act was to come in force on the 1st of January 1834. The number of negroes thus emancipated at that time amounted to 780,993. On the 1st of August 1838, there was not one slave in any British colony, excepting the Mauritius; and in this island also emancipation has since been carried into effect.

Meanwhile, though this large sum was paid in order to atone for the effects of a great national iniquity, and to suppress a trade which could not be carried on without the greatest injustice and cruelty to the natives of Africa, yet this decided step was neither the end nor the beginning of the end. Though the example was set by the British Government at a great sacrifice of money, of men's lives, and of all that is most valuable to a country, the open resistance of other governments, or their secret connivance, rendered the security and protection of the Africans as remote as ever. Under the flags of Spain and Portugal, and that of Brazil, it is said, large cargoes of slaves were from time to time transported from Africa to various ports in America and the West India Islands; and the tradė appeared to proceed as vigorously as ever. It was indeed pursued under great dangers and frightful risks to those engaged in it; but this, instead of contributing to deter the traders, rendered them only more cunning and more determined, and rendered the condition of the slaves infinitely worse. After the emancipation in the British colonies, and in consequence of the vigilant manner in which the African coast was guarded and watched, the value of slaves was so much increased, that to seize, obtain, kidnap, purchase, and transport them to the continent of America and those West India Islands, as Cuba and St Thomas, not under the British sway, was a more lucrative and consequently more tempting trade than ever. The natives of the coast have been sold or kidnapped in as great numbers as the vessels would contain; and though often slavers were captured, the value of one cargo which escaped was only so much enhanced, and it was sold to greater advantage in the market. This, however, was trifling compared to the tortures inflicted on these Africans, as we shall see. As it was illegal to seize a vessel unless with slaves actually on board, and as vessels, though well known to be slave-ships, if found without any slaves could not be touched, it often happened, that when these robbers were descried and sure to be captured by the British cruisers, the slaves were thrown overboard, in order to destroy the legal evidence of the crime. In other respects, the negroes thus kidnapped or purchased, were invariably treated in a manner much more cruel and unfeeling than when the traffic was recognized by law. In short, though the slave-trade was placed under most severe restrictions, it was by no means suppressed, nor is it yet suppressed, or likely to be suppressed.

Mr Leonard records a transaction of this nature, which well shows the reckless character of slave-traders. On the 10th September 1830, the tenders Black Joke, Lieutenant W. Ramsay, and the Fair Rosamond, Lieutenant H. E. Hardy, chased into the river Bonny, and captured the Spanish brigs Regulo and Rapido; the former of 175 tons, eight large guns, fifty-six men, and 204 slaves; the latter 147 tons, five large guns, fifty men, and two slaves; both bound for Cuba. Both vessels were discovered at the entrance of the Bonny, having just sailed thence, and when chased by the tenders, put back, made all sail up the river, and ran ashore. During the chase, they were seen from our vessels to throw their slaves overboard by twos, shackled by the ankles, and left in this condition to sink or swim. Men, women, and children were seen in great numbers struggling in the water by every one on board the tenders, and upwards of 150 perished in this way, without any means of help; for before the tenders reached the spot, all had disappeared excepting two who were saved. Several managed with difficulty to swim ashore, and many were thrown into large canoes, and in that manner landed and escaped death. The number of dead bodies, however, cast on the beech during the succeeding fortnight, showed the extent of this wholesale murder. The Rapido was with difficulty condemned on the evidence of the two surviving slaves. On board the Regulo, only 204 slaves of about 460 were found remaining.

These atrocities are said to be not uncommon. They are even perpetrated at sea, where there is no chance of the slaves escaping with life.

The mortifying part, however, of this history of the anti-slavery crusade, is the fact of the enormous waste of human life and English capital at which all this abortive undertaking is carried on. Not only is it found that to suppress the slave-trade completely is an impracticable thing in present circumstances, but it is fully ascertained that in making the attempt, hundreds of our seamen are annually destroyed in the dangerous and unhealthy service. To add to all this, the service has been carried on at an enormous expense. It was lately calculated that if all is included, the decimation of ships' crews, the constant relays required to be sent, from time to time, to the station, and other dependent expenses, a sum of not less than L.1,200,000 sterling is annually spent in guarding that part of the south coast of Africa where the slave-trade is conducted. If we suppose that only one million has been spent annually since the beginning of 1834, when the slaves were emancipated, at the lowest computation fourteen millions of money have been, it may be said, thrown away, independently of the twenty millions given for compensation. If the previous years from 1820 be included, in all probability L.48,000,000 sterling, probably nearer L.50,000,000, have been spent in this hopeless, and, it may be said, endless labour.

Waste of public money, however, belongs to the Chancellor of the Exchequer and the public at large. Our province is chiefly with the waste, the unprofitable and reckless waste, of human life. It is to be dreaded that in either case no attention whatever will be given to any statements and remonstrances that may be made, however forcible and however pointed. This is not the first time that we directed public and professional attention to the folly and ignorance of attempting either to found colonies or make cruising stations on the African coast, or, in short, in any manner to expose natives of the British islands to the pernicious climate of that region. The business nevertheless proceeds, and with exactly the same success, as that with which it has on all previous occasions been attended. We do not, however, claim all merit to ourselves. The Lords of the Admiralty appear to be aware that the African coast service is pregnant with mischievous consequences. They applied to Sir William Burnett for a Report embodying the Medical Returns from the African station for a period of time sufficiently lengthened to give correct results; and Sir William Burnett directed Dr Bryson to examine all the returns from 1820, and give from them and his own personal knowledge, whatever information might be useful in elucidating the nature of the diseases prevalent, and the causes on which their presence and activity might depend.

From this Report we are now to present a view of the sickness and mortality for the last twenty-three years.

The Report commences in 1823. During the ten years between 1823 and 1833, the traffic in slaves was vigorously prosecuted under the flags of several nations in different parts of the south-west African coast, from Gambia on the north (N. L. 13 °15′), to Benguela in 12° on the south of the equator. From certain clauses in the treaties then in force, the right of capture did not extend to vessels sailing to the south of the line under Brazilian colours, where, for the sake of security, they were generally assumed, the operations of the British cruisers were principally, though not entirely, confined to the northern latitudes between Cape Verde and the equator. The squadron at that time seldom exceeded ten, twelve, or fourteen pendants, including tenders; and the duties required from so small a force upon a line of coast so extensive, were not only at all times arduous and severe, but frequently of a most harassing and dangerous character, particularly to the subordinate officers and men. The chief slave-exporting stations were constantly kept under the strict inspection of one or more cruisers stationed opposite; and while so employed, it was not unusual for such vessels to be obliged to detach their boats on exploring expeditions, for several weeks at a time, examining creeks and rivers at a considerable distance along the coast, or lying concealed under mangrove thickets, in readiness to pursue the slave-craft, if she shipped her cargo or attempted to leave the land. All vessels, whether encountered at sea or at anchor along shore, were instantly boarded, if the sailing abilities of the cruisers permitted, for the purpose of ascertaining their character, or obtaining information. A strict look-out was maintained from each mast-head during the day, and from the bulwarks during the night, by six men, stationed, two on the bows, two on the waist, and two abaft on each quarter; while the officer on duty was enjoined to examine the horizon with a night-glass every five minutes of his watch. These duties are performed by the squadron on the South African station at the present day, with equal diligence, and as nearly as possible in the same manner.

Seldom of late years have any vessels been stationed to the northward of the Gambia, as it is understood that the slave-trade on that coast has given place to others of more humane and legitimate character. Care only has been taken that one of the squadron should appear in the bay of Portendic for a few weeks during the season of the gum trade, as a protection to the British merchant vessels trading with the Moors. In this locality, from its proximity to the great desert of Sahara, the Harmattan or hot-wind is more severely felt than in any other part of the station; and it produces its usual effects on the bodies of men and animals, and all vegetable substances, besides conveying large quantities of impalpable sand.

Between the Gambia and Sierra Leone, one, sometimes two vessels are employed, cruising along the coast, especially off the mouths of the Nunez and Pongos, where several hordes of petty slave-dealers have from time to time resided. Formerly, when it was contrary to existing treaties to detain any vessel, though fitted for the slave-trade, unless with slaves actually on board, it was the practice for the cruisers here and in other parts to move about under light sail, or with lofty sails furled, at the distance of from five to ten miles from the land, or at such a distance as not to be seen, but in readiness to chase, as soon as any suspicious-looking vessel made her appearance from the dark line of forest that seems to skirt the shore. During the last ten years, however, the practice, in consequence of new and more efficient treaties, has been different. As soon as it is ascertained that a slave vessel is at anchor in any creek or river, the cruiser either stands in directly for her, or, should there not be sufficient water or the navigation be difficult, boats are dispatched to seize and bring her out. The latter service, protracted as it often is during several successive days and nights, under scorching heat by day and heavy dews at night, with excessive exertion and fatigue, is the cause of a large amount of sickness and mortality.

Notwithstanding the extensive alluvial swamps, by which nearly three sides of this small settlement are bounded, it is on the whole greatly less detrimental to European constitutions than Sierra Leone. The temperature throughout the year is somewhat lower. Remittent fevers take place during the whole year; but between November and April they are not of a formidable character, unless when induced under the influence of intemperance, with exposure and fatigue in the marshes, or from sleeping in the open air during the night.

From July to October is the sickly and dangerous season to Europeans; and no vessel can remain more than one or two weeks at anchor near the settlement with safety to the health of her crew. During July the rains commence, and continue to fall, with intervals of fine weather, till September. The river is then greatly swollen, and its waters, tawny and turbid, from admixture with soil, overflow the banks, and inundate the marshy flats behind the settlement to a great distance, forming shallow lagoons and pools to be evaporated during the ensuing months. It is during this season, therefore, that fevers assume a virulent and dangerous character.

It is unnecessary to follow the author in his account of the coast to the south of the Gambia, which was formerly described with sufficient amplitude and detail in the account of the work of Mr Boyle. We shall merely observe that Dr Bryson adds his testimony to that of many other observers, as to the hurtful effects of the permission to go ashore occasionally granted to sailors, and the consequent indulgence in spirituous liquors and all sorts of intemperance, as a fertile source of disease. A regulation of the service allows only the officers and negroes to go ashore;—the former being understood to have sufficient self-control to resist temptation and avoid indulgence; while the latter are known to suffer either little or nothing from sleeping on shore. This regulation, however, is sometimes not enforced; and hence the evil results. Sleeping on the ground in negro huts or similar places, after free indulgence in the use of spirits, is invariably injurious; and sleeping on deck in similar circumstances, which many seamen do, is not less pernicious in its effects in that climate. (P. 10.)

In speaking of the cause of fevers at Sierra Leone, Dr Bryson thinks that, as vessels usually anchor in the open stream about half a mile from the shore, and in front of the watering-place, the position might be supposed beyond the reach of ordinary paludal exhalations. The nearest part of the Boullom shore, the most productive of fever, is at least five or six miles distant; and though this has the appearance, when viewed from the harbour, of being, and has been, described as one continuous densely-wooded swamp, yet it is not so. Behind the thicket which crowns the bank of the river, the woods are open; and towards the west, in the neighbourhood of the village of Medina, are many tracts of land under cultivation. The village itself, though close to the river, is considerably above its level. The author therefore thinks it difficult to believe, that any noxious animal emanations from this shore can have the power of affecting the health of Europeans resident in vessels, or on any part of the shore on the opposite bank of the river.

On the southern bank, the Freetown side, he thinks it still more difficult, or rather impossible, to discover any probable source whence fever might arise. The town is built on the base and side of a rocky hill, denuded of bush, and incapable at any time of the year of retaining moisture. The hills to the west are, as far as the sea, of the same geological character; all natural wood is now nearly cleared; and much is under cultivation. The ground eastward, though described as a swamp, deserves not that name. On the whole, Dr Bryson infers, that if marsh effluvia are in any way concerned in the production of the fevers of Sierra Leone, which, he thinks, evidently they are not, they must be conveyed by the wind from the upper part of the river, at its junction with the Bunce, or from the banks of the latter,—a distance, he thinks, in traversing which they ought to become quite harmless.

All this may be perfectly correct. Yet the fact is unquestioned, that endemic fever prevails at Sierra Leone at all times sporadically, and in certain seasons acquires an epidemic character, attacking those whom it had previously spared, and evincing unusual virulence and mortality; and it is not less undoubted, that it occasionally appears in the vessels anchored in the river with great violence and severity.

All that we have to say further may be said very shortly. It seems a probable inference, that African fever is not in all instances and at all times a disease the result of miasmatic and telluric influence alone. Often it appears to be a disease of atmospherical origin. The heavy night dews and the smokes appear to be as deleterious to the European constitution as the intense solar heat. But it is quite unnecessary to pursue this question, on which it is difficult to furnish any thing but conjectures; more especially as it was sufficiently examined in the account of the work of Mr Boyle, already referred to.* One fact alone mentioned in that article, and repeated in various works since that time, shows that it is not mere marsh effluvia, but something in the excessive moisture, that causes fever on the African coast. This is that, though fever may occur at any time of the year, yet it is always most prevalent and most virulent during the rains and in rainy seasons, and less so during the dry season. Now, the effect of the rains is to submerge completely the greater part of the surface, and, if they act as in other parts of the globe, they ought to extinguish and annihilate all miasmatic and telluric emanations. In all situations where marshes are productive of fever, it is not the wet but the dry and drying season that is most sickly and hurtful. As soon as the surface is put under water, epidemic influence diminishes and becomes inert.

Sierra Leone is the great station whither all slavers captured in the neighbourhood are brought, adjudged, and condemned, and where accommodation and sustenance are in the meantime provided for the liberated Africans. It may be proper to give from Mr Leonard, surgeon to the Dryad, an account of the process pursued in this case.

"As soon as an illicit trader," says Mr Leonard, "in slaves is taken possession of by one of our ships of war, which is generally done after a long chase, all her crew, with the exception of the captain and one or two others, are removed on board the capturing vessel, from which they are usually landed on the nearest part of the coast, and two midshipmen or other junior officers, and from five to twenty men, according to the size of the vessel, are sent on board to navigate her to Sierra Leone, where all slave vessels captured on the coast of Africa by our cruisers are immediately carried for adjudication by the courts of mixed commission resident there. These courts, under the provisions of the treaties between Great Britain and Portugal, Spain, the Netherlands, and Brazil, ought to consist of a commissary judge, a commissioner of arbitration, and a registrar from each of the high contracting parties. On the part of Great Britain, the governor is the present acting judge, Mr Smith (a gentleman who has held various situations on the coast for a long period), commissioner of arbitration, and Mr Lewis, registrar; but at present the Brazilian commissary judge, Mr Joseph de Paiva, is the only foreign commissioner at Sierra Leone. From the decision of these courts there is no appeal. Their duties are extensive, and the contigent expenses proportionably large, the whole of which are in the first instance paid by the British Government, but one half is afterwards repaid by the several foreign governments concerned. It appears by the report from the select committee on the settlement of Sierra Leone, 13th July 1830, ordered to be printed by the House of Commons, that the expense of these courts in salaries and contingencies, to our own officers alone, in the year 1829, amounted to L.6097, 9s. 11d. The unhappy inmates of the holds of slave vessels brought to Sierra Leone, are landed and lodged in the Liberated African Yard, as soon as it is ascertained beyond a doubt that the vessel has been legally captured, which is sometimes not done for several days; and the slaves continue cooped up in their filthy and wretched abode, until all the tedious paltry ceremonies of the law are punctiliously attended to; but if any epidemic prevails among them, which very frequently happens, they are landed, on the representation of the surgeon to the courts, immediately on the vessel's arrival, and lodged in the lazaretto, near Kissey. Here they remain until recovery, and until arrangements are made for locating them. After adjudication, a portion of ground, generally in the vicinage of one of the numerous villages in the colony, having been marked out by the government surveyor, or other person appointed by the governor, sufficiently large for the purpose of erecting huts and maintaining the newly-manumitted slaves, they are taken to the spot by the superintendent or an assistant, and employed in clearing it, and in cutting wood for building and grass for thatching their future residences; and while so employed, they are lodged in a depot in the village, or in the houses of the inhabitants, if they choose to receive them.

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* Edinburgh Medical and Surgical Journal, Vol. xxxvii. p. 146, 154, 156. Edin. 1832.

"As the latter usually find relatives or countrymen among the new comers, they are generally willing to afford them both shelter and assistance. Sometimes they are dispersed among the different villages, instead of being located in one spot. During the first six months after their arrival in the colony, they are fed and clothed by government, each receiving for this purpose twopence per diem, which is found quite adequate to their wants; and after having completed the erection of their huts, which it takes but a short time to accomplish, they are employed at any public works that may be going forward, being permitted, during part of the six months, to cultivate the piece of ground allotted to them; the assistant superintendent of liberated Africans, before leaving them entirely to their own guidance, supplying them, from an extensive depot or store kept for that purpose, situated in close proximity to the slave yard, with articles of dress and cooking utensils, together with a quantity of esculent seeds and plants, such as Indian corn and cassada, to rear for their future support. They are all much gratified on receiving these necessaries, considering themselves enriched.

"The articles at present supplied to each male emancipated slave on his location cost about L.1, 10s. which, together with his six months' allowance of twopence a-day, make the whole of the mere personal expense of each male adult to his Majesty's government amount to about L.3. The daily allowance is, of course, extended in the cases of persons who from age or infirmity are incapable of supporting themselves. Females receive twopence a-day for three months only, and as many of the children as possible above a certain age, on condemnation of the vessel, are apprenticed out, as has been already stated, to persons of respectable appearance in the colony. With the exception of those negroes recently arrived, who, from the excessive crowding, and the bad quality and scantiness of the food and water, are almost always filthy, emaciated, and covered with disease, the manumitted slaves appear in general to be clean in their persons, sleek and well fed, and very well satisfied with their condition. After a short stay in the colony, the industrious are occasionally permitted to cultivate patches of waste land in the country besides their own allotted piece of ground, with the understanding that their occupation of the former shall be temporary. By selling the produce of this they are enabled to obtain many of the comforts and a few of the luxuries enjoyed by their European neighbours."*—Pp. 84–88.

As to their subsequent habits, Mr Leonard gives the following account.

"A great many of the liberated Africans are employed as labourers in the wood trade of the river, receiving five dollars a-month as wages. Many more have been taught to employ themselves as artisans, and several are engaged daily as labourers in Freetown, and in the different villages of the Peninsula. From all that I have observed, there appears to be no lack of industry among those who have been some time in the colony, and little can be expected for a considerable period from men just escaped to light and liberty, from the dreadful privations of a slave-hold. They are acute and active in bargaining, and they do not appear to be by any means deficient in intelligence."—Pp. 90, 91.

He adds that they pay great external attention to the observance of the Sabbath; and that he never saw one liberated African in a state of intoxication.

Between Sierra Leone and Cape Mount two vessels have been generally stationed, with instructions to watch the rivers Sherbro and Gallinis, on the banks of the latter of which, about 100 miles to the south, a noted slave mart has long been maintained. Both of these, which often require to be entered by boats, are extremely prejudicial to the health of Europeans, at whatever season of the year. Their banks are similar to those of the Numez and Pongos; while the physical characters of the surrounding country are similar and equally detrimental to health.

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* Records of a Voyage to the West Coast of Africa in his Majesty's Ship Dryad, &c. in the years 1830, 1831, 1832. By Peter Leonard, Surgeon, R. N. Edinburgh, 1833. 12mo.

South from Cape Mount the coast runs in a line almost south-east to Cape Palmas, in latitude five degrees north, forming a long tract called the Grain coast. From Cape Palmas it makes a bend or sinuosity to the north, forming the Ivory coast, to longitude two degrees west of Greenwich, where the gold coast may be said to commence at Cape Coast Castle. In the whole of this tract, between Cape Mount and the Gold Coast, there are no slave marts of importance; and inspection is there accordingly less rigorous than at other points.

As far south as Cape Coast Castle also and Accra, the surf is so violent and dangerous, that it is impossible to communicate with the shore otherwise than by native canoes. This at once renders the stay of the cruisers shorter, and gives less occasion for delay; and seamen very rarely have liberty to go ashore.

To the south of the Gold Coast, the country resumes the appearance of an extensive alluvial plain, through which flows the Volta, a large river in appearance, but of short course, like all others in this tract, until we reach the Bight of Benin and the outlets of the Niger. This river being shallow is rarely entered. Both the Volta and its tributaries form lagoons in the interior, and render the whole district humid and marshy.

Next comes the Bight of Benin, extending from 2° to 9° east longitude, and in from 5° to 7° north of the equator. The first great slave mart is Whydah, and from this point southward as far as Loango, the trade of selling and exporting slaves is most vigorously carried on along the whole coast. Twelve miles farther down the coast is Badagry, a minor slave-hold; and further south, in the centre of the Bight, lies Lagos, a place equally noted as Whydah for the number of slaves exported. This tract has been long known as a great slave-supplying district, so much so, that it has been named the slave coast; and it retains, in a great degree, this character to the present day. Numerous creeks and rivers, and especially the outlets of the Niger, afford means both for bringing down slaves from the interior, for concealment, and for getting them exported.

The service in the Bight has accordingly been irksome and laborious to an extreme degree. During the earlier years included in the present Report, that is, from 1820 to 1830, when empty slave vessels were permitted to rove along the coast with impunity, until an opportunity for securing a cargo arrived, it was not unusual for cruisers to spend two, three, or four months in succession, standing off the land during the night, and on land at early dawn, until the vessels at anchor could be counted from the mast head with a good glass. The reason of this was, that slaves were seldom embarked till evening; and embarkation, which occupied from one to two hours, according to the nature of the bar and the surf, having been accomplished, the vessel immediately made sail, and endeavoured to gain an offing beyond the cruiser, if possible, before day-break.

Next to the Bight of Benin comes the Bight of Biafra, as a slave furnishing station. As the principal outlets of the Niger, viz. the Nun, Brass, Bonny, and Calabar, open in the northern angle of this bight, and the Cameroons and other streams at its southern extremity, the coast has always afforded great facilities for concealing, accommodating, and exporting slaves. The slavers also can easily lie without attracting notice in one or other of these streams, until they obtain and ship a cargo. To counteract this, in general from four to five vessels have been stationed between Cape Formosa and the equator, sometimes at anchor, more frequently under weigh. These vessels repair from time to time, or by turns, to Princes Island or Ascension for supplies of water and provisions.

Besides Princes Island, Fernando Po, Anno Bon, and St Thomas, the last under the line, all in the Bight of Biafra, are occasionally resorted as stations. All are more or less productive of endemic fever; but Fernando Po, probably from being nearest to the African shore, is decidedly the most insalubrious; and in 1833 (p. 75,) the government abandoned their establishment there. It is still resorted to by private traders.

The traffic in slaves extends south of the equator, as far as the Ambriz or Logo River, in latitude 8° south. The cruisers on this station, from five to six in number, had instructions to extend their observation from Cape Lopez as far south as the point of St Philip de Benguela, in south latitude 12½°; but the points most demanding inspection are the Gaboon, nearly under the line; Loango, in latitude 4°; Kabenda, between Loango and the Kongo; the mouth of the Kongo itself, and the Ambriz. St Paul de Loando, in north latitude 8°, the most important settlement on the coast is said to be no longer a slave-exporting station.

The whole coast embraced under the limits now specified may be regarded as the grand scene of slave-dealing, and as the spots to which the vigilance of watching vessels require most to be directed. It may be taken as a general rule, says Dr Bryson, that wherever wood and water are to be conveniently obtained, there also slaves are for sale. They are generally shipped, if not on the spot, at least within twenty miles on either side of it, by the boats of the country, which, under the pretence of carrying archil, the dye-stuff of the *Lichen roccella*, are continually passing up and down the coast. A constant communication is thus maintained; and the positions of the cruisers having been ascertained, the slaves are immediately marched across the country, and embarked at those places where is most likelihood of escaping. The vessels engaged in this traffic arrive under the American or Brazilian flag; those of the former being sold for the purpose, the American master and crew remaining ostensibly in possession, until the coast is clear and the slaves ready to be shipped. Brazilian vessels clear out at St Paul's de Loando, or at Benguela, for one of the intermediate ports, where they remain for an opportunity to ship cargo and escape, trusting principally to the intelligence obtained from the boats as to the position of the cruising vessels.

The Gaboon, though nearly under the equator, is considered the most healthy of the large rivers on this division of the station. The reason of this is unknown; and probably the fact is not ascertained. The surrounding country is hilly, with extensive intermedial swamps; while the banks of the river abound in brackish lagoons, everywhere skirted with mangrove thickets.

The native town of Loango is situate at the distance of six or seven miles from the sea shore, and is large, clean, and well built. It contains three English and three Portuguese factories; the latter for the avowed purpose of carrying on the slave trade. The climate is said to be fatal to Europeans; and those who survive its influence for one or two years evince its hurtful effects. Their houses are situate at about half a mile from the sea shore, in an open country, sufficiently elevated to command an extensive sea view. The soil is sandy, without marshes in the neighbourhood. Yet the merchants suffer from remittent fever and ague, with consequent enlargement of the liver and spleen.

Kabenda has been for many years one of the principal slave marts south of the equator. As a cruiser is always stationed in the offing, when it is considered unsafe to ship at Kabenda, the slaves are marched during the night either to Melemba Bay, twenty-three miles northward, or to the Monomazia bank, thirty miles south, near the mouth of the Kongo, at both of which places vessels may lie concealed under the high land.

The Kongo is so frequently visited by Her Majesty's cruisers, that, as a slave-trading station, its convenience is much impaired. The water is bad, brackish, and productive of intestinal disorders and dysentery.

At Ambriz, where the trade is carried on vigorously, the river is not easily entered, on account of a bar, on which the surf breaks so heavily that no open boat can cross it. It is said to be infested by alligators. When Ambriz is closely watched, the slaves are marched to Ambrizetta, and there shipped.

St Paul de Loanda and Quicombo are very nearly on an equality as to sickliness, both ague and remittent. It is supposed that at the latter place more than one half of the Portuguese perish annually. Benguela is still more injurious, and is believed to be the most unhealthy of the Portuguese African colonies south of the equator.

After this general view of the hygienic and morbific properties of the several stations on the south-west African coast, Dr Bryson enters on the examination of the sickliness and mortality of each year since 1823. To enter into the history of the whole details of these years is unnecessary. We shall merely advert to those which are most important.

The first which appears is that of the sickliness on board the Bann in 1823; and as of this we formerly gave an account, it is unnecessary to repeat it now.

In the Cyrene during the same year, at Sierra Leone in March, off the Gold Coast in April and May, in the Gambia in June, between Sierra Leone and the Cape Verde Islands from July to the end of October, upwards of 80 cases of fever occurred between March and September, seven of which proved fatal.

In the Owen Glendower, which arrived on the station in the early part of 1823, fever cases to the amount of 70 first occurred, chiefly in consequence of boat service in the Bight of Biafra and the rivers Bonny, Old Calabar, and Cameroons. Afterwards hepatitis and colonitis took place in nineteen cases of the former and seven of the latter. These diseases have been since that time of rare occurrence on this part of the coast.

The Maidstone arrived on the coast in May 1824, and continued healthy till June, when at Cape Coast, where ten cases of fever and twenty-three of dysentery occurred. From June to December, while she was employed in the Bights of Benin and Biafra, twenty-four cases of fever, three fatal, took place. Between June 1824 and 1st March 1825, between twenty and thirty cases of fever took place in the Victor, five fatal. In March, April, and May of 1825, while cruising in the Bights of Benin and Biafra, the crew remained healthy.

Early in 1824, the Athol arrived on the station. The crew continued healthy till she arrived at Prince's Island, where they were employed in watering. Thirty of the men wandered into the bush, crossed the island to the small town of Port Antonio, got drunk, and lay all night in the woods and the huts of natives. Twenty-one cases of fever followed, three fatal. Subsequently, in consequence of a similar outbreak at Sierra Leone, other cases took place. Forty in all.

From the reports from 1823 to 1825 inclusive, it results that sickliness prevailed to a greater extent both throughout the squadron and among the different European settlements along the coast, than is usual. But whether this arose from accidental circumstances, or from some epidemic condition of the atmosphere, there are no means of ascertaining. Dr Bryson thinks that both causes may have been united. He shows that the number of Europeans along the coast was increased, and that of many of these the habits were incorrigibly bad.

The fever was remittent and remittent-continuous, with early prostration, yellowness of the surface, and black vomit, and black dejections.

To the sickness on board of the Sybille, we directed a sufficient degree of attention when speaking of the work of Mr Boyle, and one circumstance only deserves particular notice.

On the 13th of January 1830, remittent-continuous fever—in short, yellow fever—broke out for the second time on board the Sybille, as was then said,* affecting eventually eighty-seven persons, and destroying twenty-six. This fever the surgeon, Dr M'Kinnal regarded as void of contagious property, but the officers and men as highly contagious. The commodore, from motives of humanity, prohibited all Europeans, excepting himself and the officers, from visiting the sick. The men were in the greatest alarm and despair when they saw their comrades smitten and destroyed by this distemper. With the view of dispelling as much as possible this mental depression, and convincing the officers and ship's company that the disease was not contagious, Dr M'Kinnal directed his assistant, Dr M'Kechnie, to collect some black vomit from the first patient in whom that symptom took place. About a pint was collected from a man, who died in about two hours afterwards. Dr M'Kinnal filled a wine-glass with this, and in the presence of the officer of the watch, drank it off. The experimenter immediately afterwards went to the quarter-deck, and walked about for one hour and a half, to prevent its being supposed that he had used any means to counteract its effects. This took place in February 1830, when the ship was cruising off Lugos, about 180 miles from the land. The appetite of Dr M'Kinnal for dinner was not impaired, nor did any inconvenience ensue.

This is eulogised by Dr Bryson as a most deliberate act of cool moral courage. In one sense it is so. But while the act is extremely revolting, it is quite useless as an experiment on the contagious nature of the disease. There is no reason to believe that the contagion of the disease, if the disease be contagious, resides in the black vomit. The black vomit is merely a quantity of blood-globules and hematosine, deprived of coagulating principle and broken down, issuing from the mucous membrane of the stomach and duodenum; and though the noxious or poisonous element evidently operates on the blood, yet it begins in the lungs and extends through the whole vascular system in such a manner, that when it has produced black vomit, the specific poison, whatever it may be, may be supposed to be wholly inert. The poison further appears to be a subtile effluvium or vapour, only capable of being inhaled by the lungs. In short, though this experiment has been performed by Chervin and other anti-contagionists, it appears to us not at all conclusive either on the one side or the other.

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* Edinburgh Medical and Surgical Journal, vol. xxxvii. p. 161. Edin. 1832.

Again, we know that much depends on the susceptibility of individuals; and the experimenter, it might in this case be said, was insusceptible of being acted on by the alleged poison.

The Eden, with a complement of 160 officers and men, arrived at Sierra Leone on the 2d September 1827, and sailed on the 4th of October for Fernando Po, with the stores necessary for the projected establishment at Clarence Cove, on that island. During the first quarter of 1828 ulcer was the prevalent disorder, the worst cases of which were treated on shore. Four cases of fever, two fatal, occurred. Fever became more general in April, and caused six deaths. Between the end of May and the 21st of July the crew continued healthy. But after this date, when they left Sierra Leone, with some volunteers from timber ships, who had been living most irregularly on shore, fever again broke out, and caused five deaths, and dysentery caused one.

Again health was restored; and while the vessel was cruising off the Bonny between the 17th September and the 12th October, all were well. On the 12th October, on returning to Fernando Po, the vessels received from the island five cases of fever, and two more before the 20th October, when she sailed from Fernando Po for Ascension. All recovered before they reached Ascension.

In treating at Fernando Po fever cases which affected a body of 25 marines, the opening of the temporal artery was thought proper, because the symptoms of determination to the head were manifest and urgent. The whole excepting the surgeon, viz. 24, were attacked; 8 died and 2 were invalided. From this Dr Bryson justly infers that detraction of blood from the temporal artery possesses no recommendation either as a remedy or a particular form of depletion. It is in truth a measure leading often to most hurtful consequences.

At Sierra Leone, on the 1st of May 1829, this vessel was healthy; but one man died on the 6th, and another on the 17th May. On the 20th of May she sailed, and arrived at Fernando Po on the 11th June. Between the 12th of May and the 31st, or in the course of 20 days, 41 cases of fever took place in the crew of this vessel; and of these 25 died. Nor was this all. In June 31 deaths took place, in July 32, and in August 7; while of 30 men left in hospital at Fernando Po, only 19 were alive on the 1st of December 1829. This makes 108 men[*] (Dr B. says 110[*]) dead of fever between 1st of May and 29th December 1829. Of these 50 died on board the Eden, and 50 on shore at Clarence Cove. This is about 68 per cent. It is elsewhere stated that this vessel lost 99 men among 160.[†]

The medical history of the colony or settlement on the island of Fernando Po is like that of all places on the south-west African coast, a history of almost uninterrupted sickliness, high mortality, much suffering, enormous expenditure, and complete frustration of all the objects of colonisation. The details it is needless to go over. During the year 1831, among the Europeans alone, 274 cases of fever of all denominations were treated in the hospital. Many of these occurred in men not connected with the naval service, viz. seamen from merchant vessels, who contracted the disease while their ships were at anchor off the island, or lying in rivers on the opposite coast, where they sometimes remained for months collecting palm oil and other articles of freight. There were also not a few wounded and sick men, principally Spaniards, landed from captured slave vessels. Forty of the deaths, however, from fever, occurred either in men belonging to the establishment or in seamen sent from the vessels of the squadron. The mortality from all forms of disease is very high. 384 men were admitted into the hospital; of these 79 died. The number of cases treated, exclusive of fever, is 110; of which 26 terminated fatally, being at the rate of one in four; a degree of mortality, says Dr Bryson, that has seldom been exceeded in any civilized community, from a similar number of diseases of the same nature. Nine cases of confirmed consumption, all terminating in death, were admitted; ten of dysentery, five fatal; and three deaths from ulcer.

As to treatment, Dr Bryson sees reason to disapprove of indiscriminate blood-letting, and to condemn as most pernicious the practice of giving mercury to produce salivation.

It appears from the above account that fever, though at all times prevalent on the coast and in the squadron, did not till 1829 assume what is called an epidemic character. In 1830 it was still more or less epidemic, at least as to the Plumper in November and December at Sierra Leone. The subjoined tables show the movements of sickliness and mortality for six years, from 1825 to 1830 inclusive.

[*]

May,	27
June,	31
July,	32
August,	7
September,	11
	108

[†] P. 87.

Tables I. II. and III., exhibiting the total loss sustained by the respective vessels of the squadron during the years 1825, 1826, 1827; distinguishing deaths by disease from those by external violence or drowning.

	Deaths in 1825. Mean Strength 663.			Deaths in 1826. Mean Strength 1043.			Deaths in 1827. Mean Strength 955.		
	By dis.	By acc.	Tot.	By dis.	By acc.	Tot.	By dis.	By acc.	Tot.
Athol, .	6	...	6	2	3	5	3	...	3
Conflict, .	...	1	1	5	...	5	2	...	2
Maidstone,	15	...	15	29	...	29	5	1	6
Redwing, .	9	6	15	6	...	6	Off station	...	...
Swinger, .	10	...	10	1	...	1	Off station	...	...
Esk, . .	1	...	1	2	...	2	5	...	5
Clinker, .	...	...	...	1	...	1	2	...	2
Brazen, .	...	...	...	6	2	8	Off station	...	...
Despatch,	...	...	...	2	1	3	Off station	...	...
North Star,	...	...	...	3	...	3	18	2	20
Sybille, .	...	...	...	..	...	...	2	1	3
Eden, .	...	..	...	...	..	..	3	...	3
	41	7	48	57	6	63	40	4	44

The six men lost by the Redwing in 1825 were drowned in the Teresa prize schooner, which was upset by a tornado, and went down at the same time with two hundred slaves.

Tables IV. V. and VI., showing the number of deaths from disease and accident in the squadron during the years 1828, 1829, 1830.

	1828. Mean Strength 958.			1829. Mean Strength 792.			1830. Mean Strength 667.		
	By dis.	By acc.	Tot.	By dis.	By acc.	Tot.	By dis.	By acc.	Tot.
Conflict, .	1	...	1	Off station	...	...	1	...	1
Esk, .	1	...	1	Off station	...	...	...	...	...
North Star,	14	1	15	Off station	...	...	...	...	...
Clinker, .	9	..	9	3	...	3	..	...	...
Sybille, .	5	. .	5	57	2	59	39	...	39
Eden, .	41	2	43	99	...	99	1	...	1
Hecla, .	5	...	5	39	...	39	...	...	...
Primrose, .	2	...	2	1	..	1	3	3	6
Medina, .	3	...	3	. .	...	...	4	1	5
Plumper,	...	...	...	3	...	3	23	..	23
Atholl, .	...	...	...	...	..		1	...	1
	81	3	84	202	2	204	72	4	76

Ratio per 1000.	By disease.	By all causes.
For 1825,	61·8	72·4
1826,	54·7	60·4
1827,	41·9	46·1
1828,	84·6	87·7
1829,	255·1	257·6
1830,	107·9	113·9

From the end of 1830 to the end of 1836, a space of six years, the squadron continued in a state of comparative health for the African station. The causes of endemic fever had subsided to their average annual force, excluding epidemic influence. The lowest mortality was 16.6 per 1000 in the year 1836, or only 16 men in a force of 965. The highest mortality during the same space was in 1832, when it amounted to 35.1 per 1000, or 18 men in a mean force of 512. It is singular, if any thing were singular along this coast, that the mortality of the year 1836 is not one-half of that in 1832. In 1837, however, the disease again evinced its powers; and we find the mortality rising at once to 128.8 and 129.9, in round numbers 130 per 1000 respectively in 1837 and 1838. In 1839, again, it falls to 69.6 per 1000; in 1840, to 37.4 per 1000; in 1841, it rises to 63.6; falls in 1842 to 32.3 per 1000; and in 1843 is so low as 18.2 per 1000, or 23 deaths among a mean force of 1267 men.

In 1844, when the mean force was 1715 men in 24 vessels, the total deaths from disease were only 43, or 25.1 per 1000; and in 1845, when the mean force in 28 vessels amounted to 2540, the deaths from disease were 121, or at the rate of 47.6 per 1000.

The subjoined table presents the absolute whole mortality for twenty-one years from the beginning of 1825 to the end of 1845, both from disease and accident, and the deaths per 1000 by disease for each year, during the same time.

Year.	Annual mean force.	Deaths by disease.	By accident.	Deaths from all causes.	Deaths per 1000 from dis.
1825	663	41	7	48	61.8
1826	1043	57	6	63	54.7
1827	955	40	4	44	41.9
1828	958	81	3	84	84.6
1829	792	202	2	204	255.1
1830	667	72	4	76	107.9
1831	785	22	3	25	28.0
1832	512	18	3	21	35.1
1833	562	12	10	22	21.4
1834	620	18	8	26	29.0
1835	815	19	3	22	23.3
1836	965	16	4	20	16.6
1837	815	105	4	109	128.8
1838	885	115	3	118	129.9
1839	790	55	5	60	69.6
1840	855	32	3	35	37.4
1841	1070	68	17	85	63.6
1842	1330	43	29	72	32.3
1843	1267	23	4	27	18.2
1844	1715	43	6	49	25.1
1845	2540	121	7	128	47.6
	20,604	1203	135	1338	58.4

It hence appears that the annual rate of mortality from disease alone on the African station for a period of twenty-one years was 58.4 per 1000 of the mean force employed, or 1203 of a total force of 20,604. The unhealthy character of the climate is clearly shown by placing in juxtaposition the station with the other stations from which accounts have been obtained. Thus,

South America,	7.7	per 1000
Mediterranean,	9.3	...
Home,	9.8	...
East Indies,	15.1	...
West Indies,	18.1	...
Coast of Africa,	58.4	...

Dr Bryson estimates nearly one-half of this proportional amount as the effect of epidemic fever alone, which was confined to a few vessels of the squadron during the years 1828, 1829, and 1830; again in 1837, 1838, and 1839; and that which occurred in the Eclair in 1845. Deducting the loss from epidemic fevers, therefore, the rate of mortality from all other classes of disease on the station he estimates at about 20 per 1000 of the mean force annually. He allows that this does not include those numerous cases in which attacks of disease are followed by permanent loss of health.

We presume that, in the above estimate, Dr Bryson does not include the ordinary annual mortality from fever, that is, fever of the coast, without epidemic characters; for if this be included, the number will surely be more than one-half. We should say from reading the report that it approaches to two-thirds. This, we further think, the author himself intended to say, when he estimates the mortality from all other diseases at 20 per 1000; for certainly 20 is nearer one-third than one-half of 58.4. In another place, he ascribes four-fifths of the mortality to fever. (P. 196.)

Of the 17 deaths by accident in 1841, 8 are thus distributed:—Two men of the Dolphin were killed in the boats by gun-shot wounds in an action with the Firme slave-vessel. Three men belonging to the Termagant were so severely wounded in a boat in the act of boarding a slaver, that, as the boat was swamped, they sunk alongside. Three of the Buzzard's crew were drowned by the upsetting of a boat on the bar of one of the rivers. Four men of the Iris, three of the Cygnet, one of the Forester, and one of the Persian are not accounted for. But we are led to understand that their deaths were occasioned by external injury.

Of the large number (29) of deaths by accident in 1842, it appears that 25 were caused by drowning, and only 4 by external violence. Of these 25 who perished in the waters, 14 were of the crew of the Madagascar. Eleven men were drowned on the 17th July, by the upsetting of a boat on the bar of the Coanza River, while endeavouring to communicate with a vessel inside, of which possession had been previously taken. This is a common cause of accident in the African rivers. Almost all are beset with bars of alluvial deposit at their outlets; and on these the surf breaks with such violence, that boats are often swamped; and amidst the broken water the strongest and most dexterous swimmers may be drowned; while the sharks which infest these situations are always ready to attack the men when struggling amidst the waves. The Madagascar lost three men by gun-shot wounds in an attack upon the slave barracoons at Kabenda, for the purpose of destroying these receptacles. Two men were drowned while attached to a prize, and one in the river Congo.

The Bonetta lost, in the same year, three men by the upsetting of a boat off the river Sestos. This accounts for 17 by drowning. There are still 7 unaccounted for. The Rapid lost 3, the Acorn 2, the Iris 2, the Water Witch 1, and the Grecia 1; but of these 9, four were by violence.

The author next proceeds to report on the fever of the Eclair in 1845. This portion of his performance, however, we must reserve for a separate article, as on that subject we have already said a good deal, and have yet to say something more, when considering the report of Dr M'William on the disease as it appeared at Bona Vista. We shall subjoin his general conclusion.

> "Having thus traced the fever of the Eclair from its first appearance on board at the island of Sherbro, until it ceased previously to her arrival at Sierra Leone, and from its reappearance there in a more concentrated form, until it finally disappeared some time after her arrival in England, it is impossible not to be struck with the close similarity which it bore to that of the Bann. Both vessels contracted the disease at Sierra Leone, and apparently from the same cause or causes, and under similar circumstances. In both vessels, in the course of a few weeks, it assumed an epidemic character, if it did not acquire contagious properties. The one vessel proceeded to the barren rocky Island of Ascension, a few degrees to the south of the equator, where a disease of the same character made its appearance among the inhabitants, and committed great ravages. The other proceeded to the nearly equally barren island of Bona Vista, a few degrees to the north of the equator, where, in like manner, a disease a short time afterwards broke out, and raged with equal severity. This is not the place, however, to trace the progress of the fever on the island of Bona Vista, nor is it perhaps the time, considering that the subject is still under investigation." —P. 194.

It further appears, from the present report, that epidemic fever broke out a second time on the Island of Ascension in the year 1838. Early in 1838, fever attacked the crew of the Forester at Sierra Leone; and again during the summer months in a severe form. In the Bonetta it broke out at sea, while on the voyage to Ascension, the vessel being at the time in a filthy state, and loaded with corn and yams; while it is added, that this occurred subsequently to her receiving a prize crew from the Forester, in which the disease was prevailing.

In the Water Witch fever made its first appearance after leaving the Island of Ascension in the beginning of May 1838, where the disease, which had broken out in the end (25th) of March, after heavy rains on the 16th and 17th of that month, was committing great ravages among the people of the garrison.

The following vessels had arrived previous to all this. The Ætna from Sierra Leone on the 26th January, after a long passage, during which she lost twenty-five persons, officers and men; the Bonetta on the 29th or 30th of January from Accra, after losing several men, and with the corpse of a young officer dead of the disease; the Forester on the 4th February, from Sierra Leone, having lost from fever her commander and several men on the passage; and the Raven on the 23d March, from Sierra Leone, after losing by fever several men and officers, the last on the 11th January.

From all these vessels foul linen was sent on shore to be washed. The effects of the deceased officers of the Forester, after having been spread on the beach and exposed to the rays of the sun for two days previously, were sold on the 3d March; and those of the officers of the Bonetta, after having undergone a more lengthened ventilation, were sold on the 17th of March, eighteen days previous to the fever breaking out on the island.

It is to be observed that, on the hypothesis, that the disease arose from, and was propagated by contagion, the largest purchasers ought in this case to have been first affected. This, however, was not the fact; some of those who bought most largely having been the last to be attacked; while others, who bought and used the articles, were never attacked by the disease at all. Neither does it appear that those most in communication with the invalids received from the vessels as patients and attendants in the hospital, suffered by the disease.

On the other hand, this epidemic affected mostly and first persons living in houses forming two squares, near which were pools of stagnant water, where all sorts of vegetable and animal remains were allowed to pass into a state of offensive decomposition. The evidence adduced by Dr Bryson from the surgeon of the Island Hospital shows, in short, clearly, that the disease was of local origin.

A few observations in reference to the concluding portion of the report on the causes of disease on the African station, and the means of diminishing and preventing, if possible, the amount and prevalence of disease, we have now to add.

In attempting to ascertain the primary and essential cause of African endemic fever, Dr Bryson thinks that all the hypotheses, whether proceeding from naval medical officers who have been on the station, or from speculators who have not, are equally unavailing to explain the phenomena of its origin, epidemic or sporadic prevalence, and cessation or occasional inertness. That it is a marsh production and aerial in form, or capable of being conveyed to short distances by the wind, he is willing to admit to be established by the fact, that fever is more generally prevalent in these localities than in those which are dry. He observes, on the other hand, that in certain marshy localities fever does not prevail; and that in certain dry sterile soils it is found identical in type and equal in violence with that of the marshes. He thinks, therefore, that it may be inferred that the cause of fever, whether in Africa or elsewhere, is not absolutely and essentially a marsh production, but rather that it exists equally in dry and marshy places; though in the latter, from some contingent local cause impairing the general health, such as abound on all parts of the coast, the system becomes more susceptible of its influence.

This accords very much with the doctrine regarding marsh-vapour, and the situations in which it is formed and evolved, taught by the late Dr Fergusson, and illustrated by Dr Ranken from his experience in India, and several other authors. Dr Fergusson showed, from a very extensive induction of facts, that endemic fever prevails in situations where there is no open or evident marsh; but where, with the external aspect of dryness, at a certain period of the process of desiccation of the surface, the soil is placed in the same circumstances as the margin of the drying marsh, and is then productive of fever.

Thus there are no evident marshes in the dry rocky ravines or quebradas of Spain, none in the dry ravines of Sicily, nor in the dry rocky islands of the Mediterranean. Yet in all these situations fever prevails during the summer and autumn months to a very great degree, and often with great virulence.

The anomaly, it appears to us, with deference to Dr Bryson, is, that on the African coast, it appears that excessive humidity is the cause of fevers, which in other situations, excepting one or two which were specified, in our account of the sickliness attendant on the Niger expedition of 1841–42,* seem connected with excessive dryness. In general on the African coast, when the rains commence, sickliness also commences; and the smokes which take place during or after the rains are always more or less productive of fever. So closely do these dense vapours appear to be connected with fever, that it is supposed by common observers, that when carried seaward, which they occasionally are, they bear with them the active exciting principle of fever. Of this conveying power Dr Bryson thinks there is no proof. But the fact of coincidence is, we believe, generally, if not universally, recognized.

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* Edinburgh Medical and Surgical Journal, vol. lxiii. p. 448. Edin. 1845.

It is to be observed, that though the whole African coast within the tropics is in different degrees productive of fever, yet the spots most especially so, are the outlets and banks of rivers, and the large delta of the Niger in the Bight of Benin, the last of which has been long noted for its febriferous properties. Every delta is necessarily an alluvial tract, varying in extent; and an alluvial tract not merely submerged for part of the year, but permeated almost like a sponge in its subsoil by water, however dry the surface may appear. Were these alluvial tracts more elevated above the level of the rivers and the sea, and were their substratal matters more consolidated and less spongy and penetrable by water, there is every reason to believe that fever would be less frequent, less prevalent, and less violent.

The difficulty, however, consists in explaining why these tracts should be so productive of fever in the wet season, and less so in the dry season. The delta of the Mississippi is most sickly during the summer, and often at the beginning of the dry season; and the banks of that river and its tributaries seem in like manner during the summer to be most productive of fever.

Dr Bryson ascribes considerable influence to the great reflux of sea water in the tidal rivers of south-west Africa; and it apappears certain that this has considerable effect. Sickliness is said to be most prevalent during the spring tides and immediately after than at other periods.

Elevated temperature has also an influence, which, though difficult to appreciate, must still be of some moment. From the Gambia on the north, to Benguela on the south of the equator, the general range in the hottest season of the year is between 80° and 86° in the shade on board ship. The variation is from 20° to 30°; the temperature during the winter months rarely falling below 58°. The presence of the tornado causes or is connected with a sudden descent of the thermometer, the fall being in the course of a few minutes from its maximum 86° to 75°; and the rise being gradual after the storm subsides.

In addition to the inherent properties of the soil and climate of Africa, however, there are in the service on that station circumstances which tend powerfully and directly to induce disease. Of these the boat service. The exposure, fatigue, and general danger of this service on the African coast and rivers it is impossible for any one to imagine who has not been an eye-witness. In order to convey some idea of the nature of this service, the author gives the following account of a boat expedition, from the medical reports of the Owen Glendower frigate.

"The assistant-surgeon being unwell, the surgeon took his place in the boats. As the ship was in the Bight of Biafra in June, either under weigh or at anchor off the mouth of one of the large rivers; and as the weather was exceedingly bad, and the men constantly employed in the boats, they had suffered much from fatigue, poor diet, and exposure to atmospheric vicissitudes. In these circumstances they were again called on to ascend one of the branches of the Bonny."

"In the eight boats were 154, officers and men. They left the ship on the 30th June, being then about 25 miles from the bar of the river. At 3 o'clock in the afternoon it commenced raining, accompanied with much thunder and lightning, and so continued without intermission until the evening of the 2d of July. The atmosphere was so thick, that the boats were obliged to come to a grapnel outside the bar. Being thoroughly wetted, a very disagreeable chill sensation was experienced by many of the party; it was felt to increase after the cessation of the rain, the air being then saturated with dense vapour, which prevented their clothes from drying; about noon, however, on the 2d, (3d?) this was dispelled by the rays of the sun, when they began to dry rapidly, to the great comfort of the sufferers. They crossed the bar and spent the afternoon in the creeks of the Bonny, examining French vessels, all of which were employed in the slave trade."

"In the evening they again came to a grapnel at the end of the flats, the weather being calm, serene, and beautiful; every one immediately stretched himself the best way he could, and went to sleep."

"About midnight they were aroused by a tremendous thunder storm, with torrents of rain, which continued without intermission until after day-light. The morning was foggy and raw, until the rays of the sun about 11 dispelled the mists and once more dried their clothing. The thermometer then stood at 80° in the shade. About 2 in the afternoon, after a long and tiresome pull of about 40 miles, they reached the object they were in search of, namely, a Spanish schooner, with 191 slaves on board; which, though she made some slight resistance, was speedily captured. After disposing of her crew with a view to safety, they made arrangements for the night, when the men, worn out with fatigue and over-excitement, threw themselves down to sleep, some on the deck of the vessel, and others on the thwart of the boats."

"The night was clear and the dews heavy; the thermometer 76°, but falling to 74°, on the morning of the 4th, which was also characterized by a dense vapour floating in the atmosphere, until near mid-day. Five of the men during the night were attacked with vomiting and purging, accompanied with severe pain at the pit of the stomach, general debility, quick pulse, and a white tongue. A scruple dose of calomel with two grains of opium soon allayed the most urgent symptoms, and a mild purgative afterwards effected a cure. As soon as the sun's rays broke through the fog, the weather became fine, and not too warm, the thermometer being only 78° in the shade. This day was spent in forming treaties with the king of Calabar. Three other men were attacked in a similar manner to those on the previous night, but were speedily relieved by the same means."

"The morning of the 5th, like the preceding, was again ushered in with fog. Towards noon they began to move down the river; but later in the day, the schooner, through the ignorance of the pilot, got aground among the breakers. Their efforts to get her off having failed, about midnight it became necessary to attempt to save the slaves, which was effected with great difficulty, as they had to be taken out of the vessel by the jib-boom, shortly after which she went to pieces. The party had now neither provisions nor water, these having been embarked in the prize, and the boats were so crowded that they could barely swim, Their situation consequently became both dangerous and distressing, particularly as they did not reach the frigate till the 7th, having been upwards of twenty-four hours without either food or water. This is not given as a specimen of a disastrous expedition; there have been many worse; but to bring more distinctly under observation the true nature of the service and the various incidents peculiar to it in the order in which they occur."—Pp. 204–206.

Dr Bryson condemns very strongly the regular issue of spirit rations to seamen on the African coast. It is, indeed, as he says, a practice most hurtful to the seamen, and is not merely a cause of acute disease, but invariably tends to render the men less capable of resisting the deleterious effects of the climate, and either kills or brings on them premature old age.

The author concludes with a judicious view of the means of preventing disease, and the methods of treatment when disease is established.

As to prevention, it may be safely said that little is in the power either of officers or men, or medical officers. Regularity, temperance, protection from smokes, night dews and rains, and from excessive solar heat, the abridgement or annihilation of boat service, with the occasional use of cinchona or sulphate of quinine, form the principal means of precaution which can be employed with hope of benefit.

These two agents, especially the latter, he represents to be by far the most energetic and successful means of preventing fever and counteracting the noxious effects of the atmosphere of the coast and rivers. The latter, indeed, is regarded everywhere on the coast of Africa a sort of necessary of life. The author adduces many examples also of the antimiasmatic power of cinchona administered in wine to the men when engaged in dangerous and insalubrious service.

Tobacco, to which some similar power has been ascribed, he regards as inert and quite useless.

As to the mode in which fever is generated or propagated, Dr Bryson several times expresses the opinion, that all the facts concur in showing, that it depends on the soil and climate, though in what manner it is difficult to say. The existence and influence of contagion he does not admit. But he allows that a fever originally endemic may possibly, under certain circumstances of crowding and want of proper ventilation, acquire contagious properties, and appear to be propagated from one person to another. On this point, however, he expresses himself with great caution.

As to treatment of African fever, when established, Dr Bryson recommends promptitude, activity, and judgment, but not excessive depleting measures. In short, it appears from the testimony of most of those who have recently treated this fever, that the full blood-lettings at one time recommended for tropical remittent by no means suits the African fever. On the contrary, the mildest measures seem to be most effectual. The blood when drawn is repeatedly mentioned to have shown little or no serum, to present a soft flaccid clot, never almost to show any buffy coat, and to be all over and throughout of an unusually dark colour. These phenomena show that the blood has lost much of its coagulating power, an effect common to all fevers, and which is perhaps to be traced, in a great degree, to the presence of a poison, and the imperfect and defective action of the function of respiration. These conditions cannot be removed by depletion. It was found further, by repeated trial, that however much one blood-letting might seem indicated by the symptoms, invariably after the second the prostration of strength and the depression of all the vital powers were extreme. For all these reasons, and the apparent inefficiency of the practice, the remedy of large blood-letting has been rather falling into disuse, and repeated blood-lettings have been viewed as not only doubtful, but positively pernicious by most of the surgeons. Dr Bryson further mentions (p. 64) a remarkable instance in which, in consequence of the illness and subsequent departure of the surgeon, the medical charge of the crew of the Eden devolved on the captain, whose knowledge and long experience of the African climate and diseases are represented to have well qualified him for performing this duty. His treatment, though simple, was more successful than any other that had been hitherto adopted. Having witnessed the evils of the energetic depleting treatment of the regular practitioners, he had imbibed a sort of horror at blood-letting, and at the same time a predilection for milder measures, probably from observing the greater success which attended the treatment pursued by the natives and resident Europeans. Abstraction of blood, therefore, formed no part of his method. He commenced with the exhibition of some brisk purgative; he waited for a remission of symptoms, when he gave quinine, and continued its use until the patient got well; omitting it, however, when a paroxysm of fever intervened. When diarrhœa took place during convalescence, which was not unusual, he gave calomel until ptyalism was fully established; after which the patient generally recovered rapidly.

The only part of this method of treatment the propriety of which appears questionable, is the exhibition of calomel to the production of ptyalism. From the evidence adduced by Dr Bryson from the reports of the medical officers, and also from the testimony of Mr Leonard of the Dryad, no fact appears to be better established than the utter inertness of mercury as a remedy against remittent fever. The treatment at Fernando Po, on board the Plumper, the Eden, and Eclair, all tend to prove this fact beyond the possibility of doubt.

> "Experience," says Dr Bryson, "has effectually taught us the fallacy of the doctrine which ascribed to mercury the power of checking the disease the instant the system was brought under its influence; and further, that ptyalism is not to be hastened, however great the quantity absorbed. The gums may become spongy and bleed; and the teeth may be loosened; yet there may be no increased flow of saliva, and hardly any mercurial fœtor of the breath perceptible. In this state fever may continue, collapse ensue, and death terminate the sufferings of the patient. Still the gums and mouth will exhibit unequivocal evidence of the system being saturated with the mineral, notwithstanding the non-respondence of the salivary glands. This has been so repeatedly remarked, that it would be waste of time further to insist on ptyalism being the effect and not the cause of returning health."—P. 242.

In short, when ptyalism readily takes place, it shows either that the disease is mild, or that its force is giving way.

Neither can it be said that mercury, if it do no good, can do no harm. It is no equivocal agent of this nature; and the present volume furnishes ample proof that in its day it has perpetrated great mischief.

The only remedies indeed which appear to be safe to be employed in the treatment of the remittent fever of Africa, are laxatives and purgatives in the commencement, and, as soon as may be practicable, sulphate of quinine. The latter remedy is best given in doses of from two to four grains every fourth or fifth hour during the day, dissolved in water, slightly acidulated with sulphuric acid, or in white wine. Calomel and opium are occasionally beneficial when vomiting and diarrhœa take place. But to push it to the extent of affecting the mouth is, as already stated, unnecessary and hurtful.

As to the treatment by the natives, in order to communicate an idea of its nature and effects, an account of it as practised at Delagoa Bay, on the east coast, was given from Captain Boteler's narrative of the survey of that coast in our account of the works of Dr M'William and Dr Pritchett on the expedition up the Niger, and the sickness which followed; and to that we must now refer the reader.* We shall merely subjoin from the same narrative a short extract regarding the experience of French practitioners on the east coast, on the inutility, or rather the bad effects, of blood-letting in treating the remittent fever of the African coast and rivers.

"In the treatment of the fever," says Captain Boteler, "the French found from experience that bleeding was highly prejudicial; they therefore relinquished it as a remedy, unless when the patient was of a highly plethoric habit. Their reason was, that although for a time it diminishes the fever, yet such is the general debility consequent upon it, that, on a second attack, nature, already deprived of her strength, sinks unresisting and exhausted. If the patient is of a bilious habit, and manifests a disposition to vomit, it is immediately promoted by a dose administered for the purpose; otherwise, in both cases, bark is deemed a sovereign remedy."—Vol. i. 157.

We cannot conclude without saying that this report is highly creditable to the talents of Dr Bryson. It presents, in short compass, a large portion of information on the causes of the sickliness and mortality of the African coast to Europeans. And it ought to convince, if any facts and arguments can convince, all who read it, that the sooner the inspecting squadron of the African coast is withdrawn, it will be better for all parties.

Art. II.—*Urinary Deposits, their Diagnosis, Pathology, and Therapeutical Indications.* By Golding Bird, A.M., M.D., F.R.S., Fellow of the Royal College of Physicians; Assistant Physician to, and Lecturer on Materia Medica at, Guy's Hospital, &c. &c. Second edition. London, 1846. Post 8vo, pp. 356.

The conditions of the urinary secretion and the deposits from that secretion have so intimate a relation with the state of the alimentary function and general heath, and are so much under the influence of the state of the alimentary function, that it is quite impossible to practise with anything like judgment the art of relieving and curing various morbid states of the human body, without giving great attention to the states of the urinary secretion. The knowledge of these states is obtained in various modes, principally by chemical examination and analysis, and microscopical inspection of the matters deposited from the secretion. For the elucidation of the nature and characteristic properties of these deposits, perhaps no one has done more in his time than Dr Prout;

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* Edinburgh Medical and Surgical Journal, vol. lxiii. p. 451. Edinburgh, 1845.

for not only has he given very accurate information on these deposits, but his example has been followed by many able and skilful disciples. We took occasion, in our sixty-fifth volume, to direct attention to the labours of one of these well-informed followers of an able master, in the short but useful treatise by Dr G. Owen Rees. The present volume by Dr Bird is another treatise of a similar character, entirely and solely devoted to the history of urinary deposits in all their forms and relations, and therefore more ample and more detailed than any that has yet appeared.

Dr Bird arranges the subjects of which he treats in thirteen chapters. In the first he gives preliminary details illustrating the chemical constitution of the urine, and the modes of demonstrating the several ingredients of that secretion. This he does both by chemical history and the use of chemical tests, and by microscopical examination. The chapter is intended to initiate those not previously very familiar with the chemical constitution of the urine; and we can conscientiously say that it accomplishes the purpose in a clear and intelligible manner. It is concluded by rules for distinguishing deposits, and by a tabular analysis of urinary deposits, and of their crystallographic characters.

In the second chapter, the author explains the physiological origin and the physical properties of urine. In the course of this history, he estimates the value of the speculations of Liebig; and next to the doctrines originally taught by Dr Prout, he assigns some merit, though he speaks diffidently on the subject, to the hypothesis of Mulder, whose discovery of proteine furnishes plausible means of explaining many of the correlative phenomena of assimilation and secretion.

The opinions and speculations of Professor Liebig have always appeared to us so rash, and so little connected with facts, that we find it impossible to place in them any confidence whatever. The inferences and views of Dr Prout, on the other hand, have only acquired additional value from the facts which the history of chemistry has of late years brought to light. Of these inferences and views Professor Liebig has made most liberal use, often without acknowledgment; and it is remarkable that, while he has thus borrowed from Prout, Boussingault, and others, he has almost constantly gone wrong when he has deviated from them and followed his own fancies.

Mulder, the author observes, has discovered two oxides of proteine, a binoxide and a tritoxide, both formed in the animal economy, and constituting, when combined with fat matter, the buffy coat of inflamed blood. Mulder believes that the proteine of the blood reaches the right side of the heart, circulates through the lungs, and combines with oxygen, forming oxyprotein (binoxide, tritoxide, or both); that this reaches the capillaries, and all is entirely or in part decomposed; the oxygen being employed for the disorganization of worn-out tissue, the protein thus deoxidized being deposited to supply its place. If more protein is set free than is required for the growth of tissues, it passes unchanged into the veins, to be again oxidized in the lungs. The tritoxide of protein being soluble in water is better enabled to traverse the minutest capillaries than if it were merely diffused through the fluid containing it.

According to the hypothesis of Liebig, which in some degree rests on the basis of the theory of proteine, the elements of muscular tissue are carried into the circulation, combined with water and oxygen; while the latter, by its union with the carbon of the effete tissue, is supposed to aid the maintenance of the temperature of the system. On reaching the glandular structure of the liver, 50 atoms carbon, 1 nitrogen, 45 hydrogen, and 10 oxygen, with an unascertained but considerable proportion of sulphur, are supposed to be filtered from the portal blood, in the form of bile, a secretion which performs an important part in the animal economy, previous to its final elimination. The more highly nitrogenized portions of the metamorphosed tissue are separated by the kidneys from the blood conveyed to them by the renal arteries, chiefly in the form of urea and uric acid, while the carbonic acid formed by the slow combustion in the capillaries of the carbon of the original atoms of muscle, is exhaled from the surface of the skin and pulmonary membrane.

This explanation would be perfectly correct were it the fact, that the theory of protein is established on a solid foundation. This, however, is very far from being the case. It is known, for instance, that albumen, fibrine, caseine, and similar substances, which are supposed, according to this hypothesis, to present, after deducting the sulphur, phosphorus, and salts, an identical atomic constitution, do not possess this identity; for the analyses of Gay-Lussac and Thenard, of Michaelis, of Vogel, and of Fellenberg, concur in showing that fibrine contains more nitrogen than albumen does. Secondly, the amount of proteine obtained from fibrine, albumen, and caseine is very variable, showing that other products not yet examined are at the same time formed. Thirdly, though, according to the hypothesis of Mulder, proteine contains no sulphur, recent researches by other chemists show that a substance free from sulphur cannot be obtained by the process of Mulder. It appears, in short, that the proportion of sulphur contained in albumen, fibrine, and caseine is three or four times greater than it could be, were the hypothesis of Mulder correct.

In short, it appears that the whole hypothesis of proteine rests on a foundation much less stable than has been supposed by the author and his followers; and it seems very doubtful if proteine be anything else than modified albumen.

Though Dr Bird does not enter so minutely into the examination of this part of the theory of *urinopoiesis*, as has here been done, yet he admits that theories like those of Liebig and Mulder, on the nature of the chemical changes carried on in the animal body, ought to be received with extreme caution. They ought certainly to be examined with strict attention before they are recognised as established parts of physiological science. For very moderate errors in the facts may vitiate all the conclusions. We doubt indeed whether any theory of the secretion of urine can be formed without a quantitative analysis, both of fibrine and gelatine, as accurate as that which Dr Prout made of albumen, showing the atomic proportions of oxygen, hydrogen, carbon, and nitrogen which enter into each. But even with that analysis of albumen, and the corresponding one of urea, it has always appeared to us that an explanation of the secretion of urine as near the fact as any yet adduced, may be given.

The hypothesis of Liebig is further exposed to another objection, which appears to our judgment fatal to its accuracy. This hypothesis proceeds on the supposition that one secretion in the animal body goes before another, and that one secretion follows another. Thus he supposes that the biliary secretion precedes the urinary secretion, and that the latter accomplishes what the former has failed to effect or has left undone. What proof is there of this precession and succession of secretions? None certainly in what is observed in the living body and its actions. Is it not an assumption, and an assumption totally at variance with all that is known of the phenomena and processes taking place in animal bodies? It seems impossible to conceive in them anything like one secretion going before the other, and leaving part of its unperformed duty to be accomplished by different organs. In the animal body, so far as we can see and understand, all processes go on simultaneously. The blood is conveyed into the capillaries and circulates through them, depositing or allowing to be taken from it by the textures that animal element which each requires,—albumen and fibrin by the muscles; gelatin by the skin and mucous membranes, tendons, ligaments, and bones; gelatin and albumen somehow modified by the *fasciae;* albumen and fatty matter by the brain; and analogous elements by other tissues and organs. But, while all this process of accretion, deposition, and repair is carrying on, the different secreting processes are going on at the same time, and not either preceding or succeeding each other. Gastric juice is secreted by the stomach, bile by the liver, pancreatic fluid by the pancreas, saliva by the salivary glands, and urine by the kidneys.

It seems most reasonable, therefore, to infer, that at every instant at which the carbon, nitrogen, hydrogen, oxygen, and sulphur of the bile are secreted by the liver, the nitrogen, carbon, oxygen, and hydrogen forming the urea, are carried away by the kidneys; and, in short, the materials of the gastric juice, pancreatic fluid, and all the other secretions are undergoing separation at the same .time in their respective glandular organs. Yet by what means, recurs the question, is this accomplished; how are all separated from the general fluid, the blood? It would occupy more time than we can at present spare to attempt even to explain and elucidate this mystery. But the following observations may be added.

We have shown that it is a complete assumption, if not entirely doubtful, that there is in the functions and processes of secretion in the animal body anything like priority or succession; that all goes on simultaneously. Nutrition, or nutritious deposition, is the principal process; and if any one part can be supposed to precede another, it would be, that nutrition, or the deposition in the textures of the appropriate nutritious matter, precedes the others. This, however, seems impossible, at least if we look to the facts furnished by the phenomena of diseases. There are several diseases in which the appropriate or characteristic principles of the secretions are either not eliminated, or they are eliminated in unusually and morbidly small quantity. In *kirrhosis* of the liver, bile is not properly secreted by that gland, and at length it ceases altogether to be secreted. In this case the materials constituting the bile are accumulated and retained within the *akini* or glandular elements of the liver, colouring them and altering them, while these *akini* are also enlarged. Part of the elements of the bile are also retained in the blood, not as bile, but as biliary elements, and thence are diffused over the system. They produce various bad consequences, and finally, directly or indirectly, cause death. The same may be said of the fatty liver, in which the glandular *akini* are filled and surrounded with fat and oil to such an extent, that either they cease to be able to secrete bile, or by some means the biliary elements are not separated.

Again, the kidneys are liable to have their granular or secreting portion so altered and disorganized by the infiltration of fat, oil, and probably stearine, with albuminous particles, that they first cease to eliminate from the system the proper and normal proportion of urea, and they gradually lose the faculty of secreting this element at all, that is, they allow to remain in the system about three times the quantity of nitrogen which would be left in the healthy state, and they allow to escape two times and a half the quantity of carbon. No doubt can be entertained, as we shall see, that this nitrogen ought to be eliminated and carried out of the system, and that the carbon should be retained. Yet neither circumstance takes place. Does the liver or any other gland assist in accomplishing what the kidneys fail to do? By no means. The nitrogen remains in the shape of urea, causes much mischief in the system, and finally carries to the tomb the person in whom this defective secretion has taken place.

Now, in these processes two things are certain. The first is, that, as nutrition proceeds, and as the nutritive elements are added to the tissues, the materials not required, as either useless or hurtful, are instantly rejected and added to the secretions; and if not added to these secretions, their presence within the system is hurtful. The second is, that, when any one gland does not properly perform its function, no other gland is able to do so. When the liver is no longer able to secrete bile, or the chemical elements of bile, neither the kidneys nor any other gland can perform this duty vicariously, or indeed in any mode. The matter which ought to have been secreted remains unsecreted, and produces its characteristic bad consequences. Conversely, when the kidneys are unable to eliminate the three atoms of nitrogen, which go to form the urea, neither the liver nor the skin, nor the lungs, are able, in the smallest degree, to give the failing organ any assistance; the matter which ought to be secreted remains; and its presence produces all the usual noxious effects observed to follow from the imperfect, impaired, or annihilated action of a gland.

The fact now stated as to the inability of one gland to assist another in eliminating materials not required in nutrition, or hurtful by their sojourn within the system, is of itself fatal to the hypothesis of Professor Liebig, though no other difficulty stood in its way. Indeed, that hypothesis is formed in such total ignorance or in complete defiance of all the phenomena presented by secretions either in health or in disease, that it seems amazing how it ever attracted any attention. If we adhere to facts and observation, it is impossible to listen to it; and it is only by neglecting all facts together that it can for one moment be entertained.

But can chemical analysis, combined with observation of the phenomena of living bodies, and morbid anatomy all combined, contribute to throw any light on this obscure subject? Are there a sufficient number of accurate and well-established facts from which positive and correct inferences may be deduced? We have already said that it is requisite to have analyses not only of albumen and fibrine, but of gelatine; for the latter and its relation both to blood and the secretions form the great difficulty in the way of every theory of nutrition and secretion. It may, nevertheless, be worth while to see what conclusions the facts hitherto ascertained may furnish.

According to the analysis of Prout, albumen and urea consist respectively of the following proportions of chemical elements:—

	Carbon.	Oxygen.	Hydrogen.	Nitrogen.
Albumen, .	50.00	26.66	7.77	15.55
Urea, .	20.00	26.66	6.66	46.66

That is, while each contains the same quantity of oxygen, albumen contains two times and a half more carbon than urea, one-sixth part more hydrogen, and only one-third of the quantity of nitrogen.

According to Scheerer, the proportions of these elements in the different forms of albumen, from eggs, blood, and according to Weidenbusch, those in the albumen of flesh of fowl and flesh of pike, are as follow :—

	Eggs.	Blood.	Flesh of Fowl.	Flesh of Pike.
Carbon, .	50.000	55.461	53. —53.31	52.64—52.50
Hydrogen, .	7.073	7.201	7.10— 6.96	7.31— 7.26
Nitrogen, .	15.920	15.673	15.82—15.69	16.48—16.65
Oxygen,	... 22.007	... 21.665	22.29—22.28	21.76—21.82
Sulphur,			1.55— 1.57	1.61— 1.57
Phosphorus,		Ash, .	20	19— .19

These results, which are said to have been obtained by the most recent and improved methods, differ from the result given by Dr Prout chiefly in the larger proportion of carbon and the smaller proportion of oxygen; the difference being from 4 to 5 per cent. more of the former and less of the latter. It is also understood that the amount of nitrogen in albumen has been generally underrated; and it is supposed that it ought to be estimated at 16 or 17 per cent.

Fibrine contains about one per cent. less carbon, from one to two per cent. more oxygen, and less sulphur than albumen.

According to Scheerer, gelatine contains nearly the following proportions:—

Carbon, . .	50.05
Hydrogen, . .	6.47
Oxygen, . .	25.13
Nitrogen, . .	18.35

That is, four per cent. less carbon, about one per cent. more hydrogen, three per cent. more oxygen, and about two per cent. more nitrogen.

Very great numerical exactness is, however, not absolutely necessary. It is enough that it appear, that between the chemical elements of these proximate animal principles there exists a relation of multiple and submultiple, or such that, in the language of arithmetic, they may be reduced to common denominators. Now, if the oxygen be in the same proportions in albumen and urea, and the carbon and hydrogen in certain proportions more, while the nitrogen of urea is three times greater in amount than that of albumen or fibrin—it follows, that during the process of nutrition so much more carbon and hydrogen are added to the tissues, and so much more nitrogen is withdrawn or rejected from them, or prevented from being united with them. In truth, that the great use of the urinary secretion appears to be, to afford a channel for the escape of nitrogenous matters, in such union with carbon, hydrogen, and oxygen as may be most favourable to the system, the retention of which within the system would be injurious.

In contributing to this end, however, the kidneys, and probably the same may be predicated of all the glands, do not perform an active part. They appear to be passive, and to allow merely the nitrogen to pass off which is not required for the repair and maintenance of the tissues. The proof of this is found, we conceive, in the following facts:—1. In the healthy state, the experiments of Prevost, Dumas, and of Lecanu, show that urea, ready formed, exists in small quantity in the blood; and others have found also, that bile, cholesterine, and similar principles may exist in the blood. 2. If the kidneys performed an active part in the secretion of urine, that is, in the formation of urea, then, whenever they were diseased and disorganized, no urea ought to be formed. This, however, we find not to be the case. When the kidneys are disorganized and degenerated by the fatty infiltration already mentioned, urea is found in the blood. This urea, therefore, must have been formed independent of the kidneys. In both it is formed in a negative matter. The albuminous and fibrinous, and, we may add, the gelatinous tissues, under the influence of vital or vital and chemical affinities, take, by a sort of elective attraction, their necessary and proper proportions of carbon, oxygen, hydrogen, and nitrogen, and no more; and in making this selection, they leave nitrogen unattached, as it were, to be united in such proportions with carbon, hydrogen, and oxygen as to form urea, which then is gradually removed by the kidneys as it is formed, and is thus prevented from causing detriment to the system.

It has been commonly taught and believed that the urine is secreted from arterial blood. But if the representations of the vascular system given by Mr Bowman, as noticed in last volume, be correct, then it follows that the urine is secreted from blood which has undergone an arterial circulation, or which is venous.

It is chiefly to the presence of urea and salts that the urine owes its peculiar density. Dr Bird agrees in adopting Dr Prout's estimate of 10.20 as near the truth for the average.

We have seen that the use of the kidneys is to convey without the system principles the stay of which would be detrimental. Dr Bird, from a large number of observations, infers that the quantity of solids conveyed out of the system by the kidneys may amount, on an average, to from 600 to 700 grains in the course of twenty-four hours; or, making allowance for variations dependent on the amount of exercise, the kind of regimen, diet, and idiosyncrasies, it may be estimated at 650 grains in twenty-four hours. Of this, from 255 to 270 grains, or more than half an ounce, are urea, which are eliminated during the same time.

Besides urea, the urine contains uric acid, sometimes separately, more frequently in union with ammonia, so as to form urate of ammonia. It is to be observed, however, that in opposition to the idea of uric acid being ever alone in the urine, it is so insoluble as to require, according to Dr Prout, 10,000 parts of water at 60 degrees for solution, while the urine has not of water quite 2500 times the weight of uric acid. It is hence to be inferred, that there is a chemical impossibility in the uric acid existing in the free state in the urine, unless there be some other yet undiscovered means of rendering this substance at least four times more soluble in the urine than chemical experiment has shown it to be. On the other hand, urate of ammonia is soluble in 480 times its weight of pure water, and in the state in which it occurs in urinary deposits, it requires for its solution 2789 parts of urine, according to the researches of Dr Bence Jones, who further shows that the presence of a moderate quantity of saline matter, such as urine usually contains, increases the solubility of urate of ammonia.

The state of the argument, therefore, stands thus:—Eight grains and one-tenth of uric acid (8.1) are normally secreted in twenty-four hours. These require only 0.82 grains of ammonia for saturation or conversion into urate of ammonia; and the 8.92 grains of urate of ammonia thus formed may be held in solution by less than half a pint of water, or about one-fourth of the quantity separated from the blood by the kidneys.

Further, by slow evaporation of healthy urine in the air-pump vacuum—in other words, by diminishing the amount of the solvent—the urine becomes turbid from clouds of urate of ammonia, which ultimately subside in minute spherical masses on the sides of the vessel. The same effect takes place when urine of high gravity is exposed to cold.

From all these various facts the inference of Dr Prout, that uric acid must be combined with ammonia, in order to be conveyed without the system, seems in the present state of knowledge indisputable.

On the other hand, it is maintained by Becquerel and some other authorities, that the uric acid is free, and that, were it not so, a single drop of nitric acid would not be sufficient to precipitate the uric acid contained in a considerable quantity of urine, as is the case. Dr Bird justly observes that this objection is more apparent than real; for while so small a quantity of ammonia is required to render the uric acid soluble, viz. about two-tenths of one grain in half a pint, it is easy to see that, if this be neutralized, which it is by eight-tenths of one grain of nitric acid, or less than one drop, the objection entirely disappears.

How, then, are we to explain the natural acid reaction of healthy urine? Is it not owing to free uric acid? Or is it owing to the lactic acid of Berzelius, and the presence of which is maintained by Cap and Henry. To the last it cannot be owing, because that is shown to be, if we believe Gregory and Liebig, not present in urine; and what was taken for it and the lactates is now believed to be organic matter, the exact nature of which is not ascertained, but which contains both kreatine and kreatinine. The difficulty appears to be elucidated, if not fully explained, by some researches of Professor Liebig on the mutual reactions of uric acid with alkaline basic phosphates. It is known that a watery solution of the common or the basic phosphate of soda exerts an alkaline reaction on litmus paper. If uric acid be heated with such a solution, it is dissolved by reason of combining with part of the soda, and setting free part of the phosphoric acid, which is supposed to form a supersalt with some of the undecomposed phosphate. The fluid in this state reddens litmus paper. On cooling, the phosphoric acid reacts on the urate of soda, and about one-half the uric acid is deposited in fine prismatic crystals, resembling in shape some varieties of uric acid sand. These crystals are not, however, pure uric acid, but contain chemically combined some phosphate of soda, of which they are not deprived either by boiling water or hydrochloric acid. The addition of an acid to the fluid decanted causes the deposition of uric acid in tabular crystals. These facts, and the inferences deducible from them, Dr Bird thinks quite adequate to explain the natural acidity of urine, and the deposition of crystals of impure uric acid on cooling. All that this requires is to suppose that the .398 of one grain of uric acid, the average quantity existing in 1000 grains, are dissolved in about 2.5 grains of tribasic phosphate of soda, the proportion of this salt found by Simon in 1000 grains of urine.

The deposits frequently found in the urine, on cooling by evaporation in vacuo, or exposure to a freezing mixture, are, however, neither always crystalline nor composed of uric acid alone. They were shown by Prout to consist of urate of ammonia, more or less tinged with colouring matter; and they are or may be amorphous and readily soluble in warm water, which scarcely acts on uric acid. Hence another explanation, or one which accounts for the formation of urate of ammonia, becomes requisite. This Dr Bird thinks may be found in the presence of the microcosmic salt or double phosphate of soda and ammonia,* and the action of uric acid thereon. When to a warm solution of this triple phosphate, uric acid is added, urate of ammonia is formed, and phosphoric acid is evolved, either free or combined with a base, and forming an acid salt. This urate of ammonia is not decomposed on cooling, but is deposited in delicate microscopic needles, readily redissolving on the application of heat, if sufficient water be present. On adding urine to a warm solution of these acicular crystals, they are deposited on cooling combined with the colouring matter of the urine, completely amorphous, and presenting all the characters of the commonest forms of urinary deposits. This experiment seems to show that the presence of the microcosmic salt or triple basic phosphate of soda and oxide of ammonia causes the deposition of urate of ammonia, while the colouring matter renders the deposit amorphous.

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* Dr Bird terms this salt double phosphate of soda *or* ammonia. In this we have no doubt that there is some error, both neological and clerical. The microcosmic salt, which has been long known and used as a flux, is a tribasic phosphate of soda and ammonia, or rather oxide of ammonium. We therefore substitute *and* for *or*.

Dr Bird then proposes, as probable, the following explanation of the mode in which uric acid exists in healthy urine,—"Uric acid, at the moment of separation from the blood, comes in contact with the double phosphate of soda and ammonia derived from the food, and forms urate of ammonia, evolving phosphoric acid, which thus produces the natural acid re-action of the urine. If the whole bulk of the urine be to the urate of ammonia formed not less than about 2701 to 1, the secretion will, at the ordinary temperature of the air, remain clear; but if the bulk of fluid be less, an amorphous deposit of the urate will take place. On the other hand, if an excess of uric acid be separated by the kidneys, it will act on the phosphate of soda of the double salt, and hence, on cooling, the urine will deposit a crystalline sediment of uric acid sand, very probably mixed with amorphous urate of ammonia, the latter usually forming a layer above the crystals, which always sink to the bottom of the vessel."—Paragraph 63, p. 60.

On the question of the physiological origin of uric acid, the author gives the opinion of Liebig, Lehmann, Zimmermann, and Heller; but as all are more or less open to objection, it seems useless to reproduce them. It is sufficient to say that the hypothesis of Liebig, which is supported by Zimmermann, is at variance with several well-known and established facts. The professor of Giessen, in short, in his anxiety to demonstate the all-powerful and constant action of oxygen within the system, has carried that doctrine to its uttermost verge; for, if all be correct that he states to be, especially as to the influence of oxygen in effecting metamorphosis of tissue, then would it follow that man and several other animals would be in the constant danger of being oxydized to death. It may be the fact, that the constant introduction of oxygen does produce a slow consumption or combustion of all the tissues, and thus hastens the death of the animal. But on the other hand, that doctrine seems highly questionable, which, in explaining certain physiological processes in the human body, is opposed to facts and daily observation. In truth, were the views of Professor Liebig correct, the proportion of uric acid ought to be increased when the food taken is rich in carbon, and to diminish if the individual confined himself to a nitrogenous diet,or in other words, lived solely on animal matters. Now, the contrary of this is known to be fact by all observation and all experience.

What are the facts in this case? Are they not that the proportion of urea and uric acid is increased when the food *cæteris paribus* consists merely of animal matter, and diminished when the food is made to consist of vegetable matter? This was clearly shown by the experiments of Magendie and many others, and accurately ascertained by experiments by Lehmann on himself, who found that under a diet strictly animal, 11.4 more grains of uric acid were excreted in 24 hours, and 582.1 grains more urea were discharged from the system, than when living on vegetable food.

Besides urea and uric acid, Pettenkofer, a diligent German chemist, demonstrated, in 1844, the existence of a new nitrogenized body, intermediate between these two principles. This body is neutral, has a bitter, pungent, saltish taste, and is readily soluble in water and alcohol. Heated in platinum foil, it melts and burns slowly, evolving an odour of urine and ammonia. By chloride of zinc, it is precipitated from its watery solution. It contains 54.02 per cent. of nitrogen, and is said to exist in the proportion of 5 grains in 1000 of urine, exceeding one-third the weight of urea. Several subsequent experiments of Liebig seem to show that this substance, to which Pettenkofer applied no name, is the same as that called *Kreatinine.*

The colouring materials of urine have long been a source of perplexity; and at present the accounts of them do not tend to simplify the subject. Dr Prout originally suggested that urine might contain two sorts of colouring matter, one remarkable for its power of uniting with urate of ammonia, and giving that salt its fawn yellow colour. An acid had been long ago stated to exist chiefly in diseased urine, giving with certain reagents, especially hydrochloric acid, various shades of purple. This acid was called purpuric, and was supposed in these states to be united with ammonia, forming a purpurate of ammonia. It now appears that the essential cause of these purple, pink, or crimson tints is a body to which Dr Bird gave, some years ago, the name of *purpurine.* Its presence may be shown with little difficulty even in healthy urine.

If to healthy urine contained in a test-tube heat be first applied to the boiling point, and hydrochloric acid then added, the fluid acquires a tint varying according to the proportion of colouring matter present, from a delicate lilac to the deepest crimson. The most common is a pink colour like a very delicate weak solution of carmine. This substance thus existing in healthy urine in small quantity, and demonstrable by the action of hydrochloric acid, is liable to be generated spontaneously in large amount in various morbid states, mostly connected with disease of the liver, as *hepatitis*, *kirrhosis*, and similar disorders. Dr Bird regards the formation of purpurine as connected with the imperfect elimination of carbon. If so, it ought to occur also in morbid states of the lungs.

Purpurine is remarkable for the facility with which it unites with urate of ammonia; and this it was which made the urine so formed be regarded as purpurate of ammonia. Purpurine is identical with the uro-erythrine of Simon, and the uro-rhodine of Heller. Living on carbonaceous matters, such as large quantities of oil, butter, and similar carbonaceous substances, or preventing their elimination, equally may cause the formation of purpurine.

The fixed salts of the urine are so called because they are left after other ingredients are destroyed by a red heat. They amount on an average to upwards of 138 grains in 24 hours. They consist of combinations of chlorine, sulphuric and phosphoric acid, with soda, lime, magnesia, and potass. The combinations of chlorine and phosphoric acid, the author thinks, are nearly all derived from the food.

That the earthy phosphates must be derived from the food must appear probable from two circumstances—the quantity of them found in the fæces, and the quantity known to exist in various vegetable articles used as food. It appears, in short, that the soluble phosphates, sulphates, and chlorides, are excreted in the urine; while the insoluble phosphates, sulphates, and silica are expelled with the fæces.

To show what proportion of earthy phosphates may be supplied by the food, Dr Bird gives the following tabular view of the phosphates in one ounce of each of eleven articles of food.

Articles of Food.	Phosphates in 1 ounce.	Authorities.
Pease (*Pisum sativum*).	9.26	Braconnot.
Maize (*Zea mays*).	7.2	Gorham.
French Bean (*Phaseolus vulgaris*).	4.7	Braconnot.
Wheat.	4.7	Liebig.
Beans (*Vicia Faba*).	4.7	Einhof.
Potatoes.	2.35	Liebig.
Rice.	1.92	Braconnot.
Milk.	1.2	Liebig.
Artichoke.	0.96	Payen and Braconnot.
Vetchling.	0.756	Do.
Beef.	0.38.	Liebig.

"The soluble phosphates," says Dr Bird, "must be regarded as derived directly from the food, and from the albumen and other elements of the blood when in the act of being organized into muscle. The insoluble phosphates forming part of the structure of the body, derived originally from the blood, are conveyed to the urine in the process of metamorphoses of tissue. Some portion of the phosphoric acid of the urine is in all probability generated from the action of oxygen on many of the structures of the body, into the composition of which, phosphorus largely enters, as the brain and nervous system generally. But the greatest part of the phosphoric acid is, as we have seen, derived, ready formed, from without, the phosphates of lime and magnesia abounding in milk and most varieties of vegetable food; whilst the basic alkaline phosphates exist in flesh, in wheaten flour, leguminous seeds, as beans and pease, &c. From the researches of Dr B. Jones, it appears that the earthy phosphates are most abundant in the urine passed a few hours after a meal, whether of animal or vegetable food, the smallest quantity being met with after a meal of meat and distilled water. The earthy salts diminish with the time elapsing since the previous meal, and they appear to be remarkably increased by the administration of calcareous and magnesian preparations. The alkaline phosphates are most abundant shortly after a meal composed chiefly of bread, and do not appear to be materially affected by the circumstances which influence the excretion of the earthy salts. Dr Jones found that in urine voided shortly after a meal, of a density varying from 1.022 to 1.028, the alkaline phosphates varied from 6.5 to 8.1 grains. The ashes of blood contain the basic alkaline phosphates; and muscle, when incinerated, yields much phosphate of lime and some phosphate of magnesia. The alkaline and earthy phosphates, in the opinion of Liebig, are chemically combined, the former with albumen, the latter with fibrine. During the formation of muscular tissue, whilst blood is becoming converted into muscle, the earthy phosphates remain in the new-formed tissue in a state of chemical combination; the greater amount of the phosphates of soda and potass re-enter the circulation, are separated by the kidneys, and thus find their way into the urine."—Pp. 80, 81.

The only point further deserving notice under this head is the proportion of sulphuric acid, which Dr Bird thinks to be too great to be entirely explained by the fact of its presence in the food in a state of saline combination. The proportion is double that of phosphoric acid; and it may be found in abundance in the urine, when food entirely free from sulphates is taken. The presence of this acid, therefore, the author ascribes to the oxidation of the sulphur, which with phosphorus is combined with the albuminous and fibrinous tissues. As this, however, may not be sufficient to account for the whole, he looks to another source, namely, the sulphur of the bile. It is well known that, in 1846, Professor Redtenbacher announced the fact, that nearly 25 per cent. of sulphur exists in taurine, one of the products of the metamorphosis of the bile. He thinks, therefore, that a portion of the sulphuric acid of the urine may be regarded as proceeding from the oxidation of the biliary sulphur.

Sulphur in the non-oxidized form has also been shown by Dr Edmund Ronalds to exist in the urine.

The following is the list of deposits which Dr Bird gives at the close of the chapter; divided into four classes:—

Class 1. Deposits composed of ingredients formed directly or indirectly from the metamorphosis of tissues, or from the inorganic elements of food, capable of assuming a crystalline form.	Class 2. Deposits composed of ingredients of inorganic origin.	Class 3. Highly-coloured deposits, black or blue, of doubtful origin.
Uric acid and urates.	Phosphate of lime.	Cyanourine.
Uric oxide.	Ammonio-phosphate of magnesia.	Melanourine.
Oxalate of lime.	Carbonate of lime.	Indigo.
Cystine.	Silicic acid.	Prussian blue.

Class 4. Deposits consisting of non-crystalline organic compounds.

A *Organized.*		B *Non-organized.*
Blood.	Epithelium.	Milk.
Pus.	Spermatozoa.	Fatty matter.
Mucus.	Torulae.	Stearolith.
Organic globules.	Vibriones.	

On the next chapter, devoted to the pathological causes and relations of uric acid, we must be brief. The deposit takes place chiefly in persons who live much on animal food, and take little exercise, and accordingly occurs in the gouty. It may, however, take place in persons in whom there is going on a process of disintegration of tissues; that is to say, where the several elements of the tissues are separated and thrown into the circulation; and consequently in whom nitrogenous elements are present in unusual proportion. In all diseases attended with great emaciation, where the wear and tear of the frame is not compensated by the supply of food, an increased quantity of uric acid appears in the urine, if the kidneys remain sufficiently healthy to perform their functions. The chief exceptions to this rule are diabetes and fatty infiltration of the kidney. In all acute inflammatory diseases, in acute inflammation supervening on chronic malady, in rheumatism, in organic and even dynamic disorders of organs materially influencing the circulation, as the heart, liver, and spleen, a considerable increase in the quantity of uric acid appears, and deposits of it take place. In the two kindred disorders, gout and rheumatism, exclusive of the many neuralgic diseases popularly referred to the latter, a great tendency to the formation of uric acid, both pure and combined, occurs. The elements of the acid, or its combinations, are in these diseases supplied both by the nitrogenized elements of the food, and the changing and disintegrating tissues of the body. It is then that urate of soda is generated in such quantities that the watery portion of the blood is inadequate to its solution, and part of it is deposited in the joints and sheaths of the tendons, producing painful swellings, and rigidity and immobility of these parts.

It may even escape by the skin, and give rise to, or be connected with various eruptions. The author mentions a case in which he visited a patient bedridden with rheumatic gout, and in whom the legs were covered with an eczematous eruption, where, on the drying of the exudation, the parts were covered with crystals of urate of soda. Another observation adduced by the author, that in cases of lepra, psoriasis, and ichthyosis, the urine abounds more in urea than is consistent with health, we can confirm from personal observation. Not only does the urine abound in urea in these cases, but urate of ammonia is almost constantly deposited. Indeed many of our most troublesome cutaneous eruptions depend, as far as can be judged, upon the presence in the blood, of principles which ought to be excreted, either by the liver or the kidneys, but which are retained.

In considering treatment, Dr Bird arranges his curative indications under three heads: 1st, attention to the functions of the skin; 2d, restoring the tone of the organs of digestion; and, 3d, the administration of solvents of uric acid.

For accomplishing the first object, he recommends the use of warm clothing, the warm or rather the vapour-bath, and friction to the surface.

For the second indication it is necessary to diminish, and in some instances to withdraw entirely all nitrogenous articles of food, exhibiting at the same time occasional minute doses of mercury, either blue pill or the gray powder, with small doses of the carbonates of potass or soda in the compound gentian mixture. In some cases also, iron in the form of ammonio-citrate, or ammonio-tartrate becomes requisite; and meadow-saffron, as a means of causing the excretion of uric acid, is also recommended.

For fulfilling the third indication, the alkalies and their carbonates, bicarbonate and phosphate of soda, and benzoic and uric acids, are to be administered in adequate doses. The bicarbonate of potass he exhibits with citric acid in a glass of tepid water. With all this is combined the use of diluents.

In the fifth chapter the author treats of the chemical pathology of uric oxide, and its relation to uric acid.

In the sixth chapter comes the chemical pathology of purpurine, to which we have already directed some attention.

In the seventh chapter the pathology and chemical history of cystine are considered.

In chapter eighth the pathology and chemical history of hippuric acid are examined.

As these are of less importance than those deposits which we have already examined, we shall not dwell on them.

Of the ninth chapter, the subject is the chemical history and pathology of oxalate of lime. On this obscure and uncertain subject the author advances several ingenious conjectures. When we consider the relation between sugar and oxalic acid, and the possibility of converting by means of nitric acid the former into the latter, it seems a probable inference to think that by some means the saccharine matter of the food is converted into oxalic acid and then united with lime. This explanation, however, of the formation of oxalic acid within the human body the author thinks not admissible. From various facts he infers that it is more probable, that the elements of the urea may undergo a new arrangement, and by this means be converted into oxalic acid. His reasonings may be understood from the following passages.

"My own experience has led me, however, to the following conclusions regarding the circumstances under which oxalate of lime occurs in the urine.

"1. That in the urine under examination, oxalate of lime is present, diffused through the fluid, and in a crystalline form.

"2. That in rather more than one-third of the cases, uric acid or urates existed in large excess, forming the greater bulk of the existing deposit.

"3. That in all, there exists a greater proportion of urea than in natural and healthy urine of the same density; and in nearly 30 per cent. of the cases, so large a quantity of urea was present, that the fluid crystallised into a nearly solid mass on the addition of nitric acid.

"4. That the urate of ammonia found in the deposits of oxalic urine is occasionally tinted of a pink hue.

"5. That an excess of phosphates frequently accompanies the oxalate.

"6. That the existence of free sugar in the specimens I have examined is the exception to the rule.

"174. Every one is now tolerably familiar with the composition of the urine in diabetes, and it has been determined, from extended observation, that, as a general rule, diabetic urine very seldom contains an excess of urea, uric acid, or urates, especially the pink variety; and that this secretion is remarkably free from saline deposits; the increased specific gravity depending upon the presence of large proportions of sugar. In the oxalic urine under consideration, the density *increases with the quantity of urea*, which is often present in large excess: deposits of uric and urates are frequent; and, further, no analogy whatever with sacchrine urine exists, save in density, which we have already learned depends upon a totally different cause. Thus, so far as the abstract examination of the urine is concerned, not the slightest countenance is given to the idea of there being any relation between oxalic and saccharine urine, however much our preconceived and hypothetical views may have led us to expect the existence of such relation. In but few instances have I yet found sugar present in oxalic urine; and although these investigations were commenced with a strong bias in favour of the almost necessary connexion between the presence of saccharine matter and oxalic acid, yet, in proportion as I have extended my researches, this idea became less and less supported by experience. In fact, I have never as yet met with oxalate of lime in diabetic urine. I have been twice shown specimens in which a white creamy sediment was considered to be oxalate of lime; but this, by chemical examination, turned out to be a chylous deposit, containing much fatty matter, and yielding butyric acid, or something analogous to it, by distillation. What, then, is the source of the oxalate of lime? and how can its production be explained consistently with the phenomena presented by the urine? From the symptoms presented in cases of this disease, there is no difficulty in proving to a demonstration the positive and constant existence of serious functional derangement of the digestive organs, especially the stomach, duodenum, and liver; and, further, that the quantity of oxalic acid generated is, to a very considerable extent, under the control of diet; some articles of food quite free from oxalic acid at once causing the excretion of this substance in very large quantities, whilst others appear to have the effect of nearly totally checking it. These circumstances alone, together with the emaciation so generally present in the disease under consideration, at once prove, that whatever be the immediate agent which causes the kidneys to secrete the oxalic acid from the blood, the primary cause must, as Dr Prout has well and satisfactorily shown, be referred to an unhealthy condition of the digestive and assimilative functions. An excess of urea, and often of uric acid, in most instances co-exists with deposits of oxalate of lime; it is, therefore, highly probable that both these unnatural states of the secretion are produced by the same morbid influence. Further, when the very remarkable chemical relation existing between urea, uric acid, and oxalic acid is borne in mind, as well as the readiness with which the former bodies are convertible into the latter, is it not a legitimate conclusion to suppose that the disease under consideration ought to be regarded as a form of what has been aptly termed by Dr Willis azoturia (of which an excess of urea is the prevalent indication), in which the vital chemistry of the kidney has converted part of the urea, or of the elements which would in health have formed this substance into oxalic acid? This view appears to me to be supported by what I have observed of the history, symptoms, and progress of the cases, as contrasted with the changes presented by the urine during treatment. It may, however, be asked, from whence are the nitrogenised matters derived, whose metamorphic changes give rise to the formation of oxalic acid? Are they derived from the tissues of the body, like healthy urea and uric acid. Of course it is quite possible that such may be their origin, but as the quantity of oxalate of lime deposited from the urine is always the greatest after a full meal, and often absent in the *urina sanguinis*, or that passed on rising in the morning, frequently disappearing under the influence of a carefully regulated diet, and reappearing on returning to the use of unwholesome food, it is highly probably that this salt is, in the majority of cases, primarily derived from the mal-assimilated elements of food, and not, like uric acid generally, a product of metamorphosed structures.

"175. The ready conversion of uric into oxalic acid, under the influence of oxidizing agents, has been satisfactorily demonstrated by Professors Liebig and Wohler; for when uric acid is heated with water and peroxide of lead, the latter gives up part of its oxygen, and oxalic acid, carbonic acid, with allantoin, the peculiar ingredient of the allantoic fluid of the fœtal calf, are generated.

$$
\left.\begin{array}{lcr}
 & & \text{C N H O} \\
\text{1 at. uric acid} & = & 10+4+4+\ 6 \\
+2 \text{ — oxygen} & = & 2 \\
+3 \text{ — water} & = & 3+\ 3 \\
\hline
 & & 10+4+7+11
\end{array}\right\} = \left\{\begin{array}{ll}
\text{C N H O} & \\
2+2+4+\ 2 = & \text{1 atom urea.} \\
4 \qquad\quad\ 6 = & \text{2 at. oxalic acid.} \\
4+2+3+\ 3 = & \text{1 at. allantoin.} \\
\hline
10+4+7+11 &
\end{array}\right.
$$

The allantoin itself requires merely the addition of three atoms of water to be converted into oxalate of ammonia, a change readily effected by boiling with an alkali.

"The readiness with which, under certain circumstances, uric acid is converted into the oxalic, may be well illustrated by a fact which has been observed in connection with the *guano* of South America, a substance now acquiring great celebrity as a manure. This contains, when recent, a considerable proportion of urate of ammonia, which salt, after a certain length of time, often during the voyage to this country, nearly wholly disappears, and is replaced by oxalate of ammonia. The relation between urea and oxalic acid is readily shown; for if we conceive urea to exist in the blood, and it be the duty of the kidney to separate it, we have only to suppose the organ to exert a slight deoxidating or decomposing influence to ensure the conversion of urea into oxalate of ammonia. We know that under a depressing influence exerted on the nervous system at large, or upon a portion of it connected with the functions of the kidney, as during typhus adynamic fever on the one hand, and blows over, or a fracture of, the spine, on the other, such decomposing influence is unquestionable, and the urine becomes loaded with carbonate of ammonia from a re-arrangement of the component elements of the urea; one atom of urea and two of water being resolved into two atoms of carbonate of ammonia. If, then, a less energetic amount of this morbidly depressing influence be supposed to be exerted, we shall have one atom of urea and two of water, lose an atom of oxygen to become converted into oxalic acid and ammonia."—Pp. 176—180.

The treatment is given in the following manner:—

"As a general rule, the functions of the body, where obviously imperfect, should be corrected, the general health attended to by the removal of all unnaturally exciting or depressing influences, the skin should be protected from the sudden alternations of temperature by a flannel or woollen covering, and the diet carefully regulated. This has generally consisted of well-cooked digestible food, obtained in about equal proportions from the animal and vegetable kingdom; all things which tend to produce flatulence being carefully avoided. The drink should consist of water, or some bland fluid, beer and wine being excluded, especially the former, unless the patient's depression render such positively necessary. A very small quantity of brandy in a glass of water has generally appeared to be the most congenial beverage at the meals. The administration of nitric acid, as suggested by Dr Prout; or what appeared to be preferable, the nitro-hydrochloric acid, in small doses, in some bitter infusion; or, laxative mixture, as the mistura gentianæ comp., is, with minute doses of mercury, generally successful, if continued a sufficient length of time. Where great nervous irritability exists, the sulphate of zinc is often of great service. It should be given in graduated doses, beginning with one grain, thrice a day, increasing the dose every third or fourth day until 18 or 20 grains are taken daily. The addition of a grain or two of ext. hyoscyami or camphor often enables it to be better borne. The shower-bath, by acting in a similar manner, has been also of great service. Where the patient is anæmiated or chlorotic, the salts of iron, in large doses, appear to be of great use, not only by subduing the irritable state of the nervous system, but by increasing a healthier condition of the blood."—Pp. 185, 186.

The tenth chapter is devoted to the important subject of the earthy salts, explaining their chemical history, pathology, and treatment. We shall give merely the following general conclusion:

"That, where the presence of a deposit of phosphates is independent of the irritation of a calculus, or of organic disease, it is most abundant in the urine passed in the evening (urine of digestion), and absent or replaced by uric acid, or urates, in the morning (urine of the blood), the urine being always of tolerably natural colour, never below, and often above the mean density. Where the presence of phosphatic salts depends on the irritation of a calculus, or of organic mischief in the urinary passages, the urine is pale and whey-like, of a density below the average, often considerably so, and the earthy deposit is nearly equally abundant in the night and morning urine." —P. 227.

This chapter, indeed, must be studied carefully by any one who wishes to understand either the pathological relations of the earthy deposits or the method of treating them with success.

The eleventh chapter is devoted to the subject of abnormal blue or black colouring matters.

In the twelfth chapter the subject of examination is the non-crystalline organic deposits, as blood, purulent matter, albumen, mucus, epithelial scales, *torulæ*, milk, fat and oil, and similar substances. Some of these have at different times been considered in this Journal. Others are more or less known, and we need not dwell on their etiology or effects.

The thirteenth chapter is of a general character, consisting of remarks on the therapeutical employment of remedies influencing the functions of the kidneys. Under this head we shall merely give here those laws which, the author thinks, ought to be observed in the exhibition of remedies.

"*Law* 1*st*. All therapeutical agents intended to reach the kidneys must either be in solution when administered, or capable of being dissolved in the fluids contained in the stomach or small intestines, after being swallowed."—P. 318.

"289. *Law* 2*d*. Bodies intended to reach the kidneys must, to insure their absorption, have their solutions so diluted as to be of considerably lower density than either the *liquor sanguinis*, or serum of blood (*i. e.* below 1.028.)"—P. 320.

"*Law* 3*d*. 'If a sufficient quantity of water cannot be received into the small intestines, or the circuit through the portal system in the vena-cava ascendens, or thence through the lungs and heart into the systemic circulation, be obstructed, or if there be extensive disorganisation of the kidneys, the due secretion of urine cannot be effected."—P. 327.

At the close of the chapter the following conclusions, which deserve great attention in the treatment of diseases, are given.

"1. Whenever it is desirable to impregnate the urine with a salt, or to excite diuresis by a saline combination, it must be exhibited in solution, so diluted as to contain less than five per cent. of the remedy, or not more than about twenty-five grains in an ordinary draught. The absorption of the drug into the capillaries will be ensured by a copious draught of water, or any diluent, immediately after each dose.

"2. When the urine contains purpurine, or presents other evidence of portal obstruction, the diuretics or other remedies employed should be preceded or accompanied by the administration of mild mercurials,—taraxacum, hydrochlorate of ammonia, or other cholitic remedies. By these means, or by local depletion, especially by leeches to the anus, the portal vessels will be unloaded, and a free passage obtained to the general circulation.

"3. In cases of valvular or other obstructions existing in the heart and large vessels, it is next to useless to endeavour to excite diuretic action, or appeal to the kidneys by remedies intended to be excreted by them. The best diuretic will in such cases be found in whatever tends to diminish the congested state of the vascular system, and to moderate the action of the heart; as digitalis, colchicum, and other sedatives, with mild mercurials."—Pp. 329, 330.

Though we have given a rather detailed account of the contents of this work, we must inform our readers that the only effectual way to understand the merits of its contents is to study the work with care and attention for this purpose. Reading once will not suffice. The book requires to be read again and again, and kept on the table as a work of reference, to be consulted whenever questions regarding urinary deposits and their causes occur.

This treatise further contains the most ample and elaborate examination of the doctrines of Professor Liebig; and it furnishes in many points such a full refutation of these doctrines and exposition of their errors, that it will require all the ingenuity of that learned and skilful chemist to answer them. In fact, we think that the present volume shows that these doctrines are so untenable, and so little accordant with facts, that they will require to be completely remodelled and revised, before they can be again submitted to the chemical world, and those interested in correct physiology and pathology.

ART. III.—*On the Causes and Treatment of Abortion and Sterility. Being the Result of an Extended Practical Inquiry into the Physiological and Morbid Conditions of the Uterus, with reference especially to Leucorrhœal Affections and the Diseases of Menstruation.* By JAMES WHITEHEAD, F.R.C.S. Surgeon to the Manchester and Salford Lying-in Hospital. London, 1847. 8vo, pp. 326.

ABORTION may be defined to be detachment of the ovum and its consequent exclusion from the uterus, either prematurely or at the usual period at which parturition would have taken place;—involving at the same time, in the great majority of cases, cessation of life in the ovum. Sterility consists in the uterus being incapable of receiving or maintaining an ovum so as to nourish it to the usual period of parturition in the human female. These two acts or processes may be said to be the leading defects or disorders in the function of the uterus; and their causes are so manifold, and involved in obscurity so great, that it becomes a proper subject of investigation for the physiologist, as well as the scientific accoucheur, to ascertain the causes on which these defects depend.

Neither abortion nor sterility can be properly or justly denominated diseases. They are deficiencies in the function of the uterus, as has been already said; but though not perhaps entitled to be called diseases themselves, they depend on the presence and operation of certain morbid states either in the uterus and its appendages, or in the system at large.

Sterility comes first of these states, as apparently the most complete and most simple denegation of function. In it, strictly speaking, the uterus has never contained an ovum;

either because the ovaries have not sent forth any ovum; or because the Fallopian tubes have not received and conveyed any ovum to the uterus; or lastly, because the interior of the womb itself is in a state so unnatural and unhealthy, that, although an ovum were conveyed thither, it could not be attached, it could not form or receive a vascular system, or if it did, could not be animated to be the seat of vital processes.

Abortion, on the other hand, is totally different. An ovum has been sent out by the ovary, conveyed to the womb by the Fallopian tube, and has been received by the womb. It has also become attached to that organ, which has so far performed its duty, in assuming the usual vascular orgasm in its inner surface, and pouring forth the usual plastic materials to form a decidua. The ovum has further received blood from the uterine vessels, has been animated, increased in size, and developed to a certain extent in all its parts and organs. All its members have been written in the book; and according to the laws which regulate the growth of living beings in the womb, they have been gradually evolved, and grown to a certain magnitude. Here, however, they have at once or slowly ceased to grow any further. The vessels have ceased to convey blood from the womb of the parent to the members of the growing animal; or the ovum has been from some cause separated from the inner surface of the womb; and the uniting vessels have been lacerated; and death of the ovum has been the result. This occurrence usually takes place before the full time of parturition. It is accordingly an untimely birth; and hence this term and that of abortion have been often used synonymously.

What may be the cause or causes of these phenomena? This is the question which it is the object of Mr Whitehead's work to elucidate, and to explain.

To the due performance of its functions a certain state of the uterus is requisite. This condition is menstruation, an act consisting in the periodical secretion from the inner surface of the uterus of a certain quantity of blood-coloured fluid. The phenomena of menstruation are understood to occur monthly, the discharge occupying from three to seven days. Of course variations may take place; the time being shorter in some females and more lengthened in others. Some irregularity takes place, it has been long known, even within the limits of health; and though females always attach great importance to the presence and the regular return of this secretion, in its effects on health, yet probably in this, as in other processes, nature allows or at least endures considerable latitude.

With the view of ascertaining how far irregularities in men struation might be compatible with health, Mr Whitehead undertook a series of extensive inquiries; and from these he obtained results which are stated in the following manner:—

"Of five hundred and twenty intelligent, many of them educated, women, in whom menstruation had commenced favourably and been continued regularly at the periods peculiar to each, for a sufficient length of time together to afford a fair average, three hundred and fifty-nine had always menstruated regularly, no particular difference being observable either in the length of the interval between one period and another, or in the number of days during which the menses continued to flow: in several, some slight difference had been occasionally noticed in the quantity and appearance of the discharge, but unattended by any concomitant effect upon the general health. The cases differed individually one from another, however, to a considerable extent. In about one-tenth of the number of the cases last mentioned, which may be termed *regular,* the menstrual periods recurred at intervals varying from twenty to twenty-seven days (calculating always from the day of commencement of one period to that of the next following); but in the majority the interval was a lunar month. But whatever the interval, whether twenty or thirty or any intermediate number of days, it had always been the same in the same person for a considerable length of time in this group of cases. For example, a lady of the bilious temperament, of a delicate though healthy frame, and of an anxious disposition, during a period of more than two years menstruated, without interruption or deviation, every four weeks, commencing always on the Monday morning, and ceasing on the following Saturday. Another, very similar in habit of body and circumstances, commenced always on the Thursday and ceased on the following Sunday. One woman, who was employed as a hawker of earthenware goods, of a strong and healthy constitution, light hair and ruddy complexion, commenced for years on the Tuesday, the discharge never continuing longer than twenty-four hours. Eight others, of different habits of body, and variously occupied, menstruated only one day each time. In three individuals, of whom two had the bilious and one the lymphatic temperament strongly expressed, menstruation recurred every four weeks, and continued never less than thirteen days, the discharge being abundant and uninterrupted the whole time. These were all employed as mill operatives, but now solely in domestic occupations, being mothers of families. Since marriage, the health of each has been equally good as before, and no difference in the functional discharges, or their times of recurrence, has been observed, except during the periods of child-bearing and lactation. In the majority of the cases included in this group, whatever the free interval may have been, the menstrual discharge generally occupied a period of from three to seven days.

"Of the remaining one hundred and sixty-one cases, which may be called *irregular;* in fifty-one, menstruation recurred every lunar month, but every third or fourth return a difference of three or more days was observed in the duration of the discharge, and often a difference, also, in the quantity thrown off in a given time; thirty-eight menstruated every lunar month generally, but every third or fourth time from four to seven days earlier; and these deviations were so marked and constant in most instances as to be anticipated at particular times. The amount of excreted fluid was also variable in these cases; fifteen had the menses every three weeks generally, but every third or fourth return from four to seven days later; fourteen every twenty-four days, but occasionally the interval was twenty-eight days; five every five or six weeks, but having occasionally an interval of only a month; two every eighteen days, of whom one had the discharge four days and a free interval of only fourteen—the interval now and then, however, being two or three days longer, and the duration of the discharge correspondingly abbreviated; the other having every third or fourth time an interval of a month. One menstruated every lunar month, but at the middle of every third interval she had an additional discharge every way similar to the catamenial, which continued thirty or forty hours, making no perceptible difference in the regular periods. Two menstruated every fourteen days, in one of whom the discharge generally continued seven days, leaving a free interval of seven,—but occasionally the interval was ten days, and the active period only three or four; in the other case, the discharge generally continued two days, but sometimes only a few hours. One individual had the menses monthly, but every third time she missed the period, having a free interval of two months, her health never suffering in consequence. And thirty-two menstruated so irregularly as to afford no means of calculating the periods of recurrence or their duration with any degree of exactness."—Pp. 7—9.

The blood of the menstrual secretion has been generally believed to differ from that circulating in the vessels of the living body, by its not coagulating, and by this phenomenon being usually ascribed to the absence of fibrine. On this character Mr Whitehead either throws some doubt, or he states the fact in a different manner from other observers. He distinguishes menstrual blood into two varieties; one the ordinary menstrual fluid; the other the true menstrual secretion.

Of the first he gives the following account:—

"Of more than fifty specimens of ordinary menstrual blood which I have had an opportunity of examining, the following general results have been arrived at. As chemical analysis has not been attempted, I pretend to no practical knowledge of its constituent proportions. 1st, The acid nature of the fluid was unequivocal in every instance, but with varying degrees of intensity. 2dly, Its colour was similar to that of healthy venous blood drawn from the smaller vessels, and never so florid as that of the arteries: it was always less viscid than either. 3dly, It did not coagulate. In some instances, however, when the discharge, from over-exertion, mental anxiety, or confinement to a heated atmosphere, became unusually profuse, clots were not unfrequently noticed among the secretion, and these had always an alkaline reaction.

"Under the microscope it presented a considerable number of blood corpuscles, aggregated in linear or irregular groups, and floating in a pale, pinkish serum; these corpuscles, in the course of a few minutes after the removal of the fluid from the vaginal surface, appeared to diminish in number, some undergoing at the same time a change in their form and size: it occasionally contained a few lymph globules, and generally a number of small granular bodies like oil globules. It always contained, moreover, a great quantity of epithelium scales of different shapes and sizes."—Pp. 20, 21.

The latter, which he is particular in distinguishing, is obtained in a different manner, and further presents different characters:—

"The sensible properties of pure menstrual blood, unmixed with the vaginal mucus, are widely different from those of the modified fluid above-mentioned. For the purposes of examination, it may be procured by means of the speculum, care being taken always to remove, by the aid of a piece of lint or sponge held between the blades of a sponge-holder, all the secretion from about the *os* and *cervix uteri*. This must be done immediately after the instrument shall have been properly adjusted, and will be found necessary in order that the fluid may be free from admixture with that exuding from the surrounding surfaces. In consequence of the irksomeness of the procedure, however, the patient will seldom be able to remain in the required posture longer than until from ten to twenty grains have accumulated. This quantity, although too small for the purposes of chemical analysis, will nevertheless be sufficiently large for determining its sensible properties, as well as for microscopic investigation.

"Thus procured, I have had opportunities of examining more than a dozen specimens, and have found it, for the most part, possessing the following distinctive characters:—1st, It is never so dark in colour as ordinary menstrual blood, so called, nor always so florid as that of the arteries; its colour, however, seems to vary slightly, being at certain times rather more florid than at others. This difference of shade which the fluid exhibits at the moment of its exudation, is attended by no corresponding change in its appearance subsequently, after being incorporated with the vaginal mucus; the effect of this latter product upon venous and arterial blood being very nearly the same. 2dly, Its consistence cannot be ascertained in every instance, as it does not always flow in a continued stream, unseparated. When collected in its *entire* state, it appears rather more viscid than the systemic blood, owing probably to its slow exudation. 3dly, When thus collected, it invariably coagulates, the separation of the clot from the serum being sufficiently distinct in three or four minutes:

the smallness of the quantity may account for the short time required for the accomplishment of this change. 4thly, It sometimes passes off in a continued stream as pure blood. More frequently it is observed to escape from the *os uteri*, partly in form of a thin, coloured serum, and partly in flattened clots, of the size of small orange seeds, which, soon after their escape, become broken down and dissolved, as it were, in the vaginal mucus, escaping at the *ostium externum* in the usual uncoagulable fluid form. 5thly, It is invariably alkaline, quickly changing the turmeric paper to a reddish brown colour, and the litmus to a decided blue.

"Examined under the microscope it appears to be a very simple product, consisting chiefly of a number of blood corpuscles floating in a limpid, almost colourless fluid. The corpuscles are generally observed to be in greater abundance in this than in healthy systemic blood, and perhaps less clustered together in groups, and more equally distributed. There are often, also, a few white globules, larger in size than the blood discs, spherical, and nucleated, and a number of very minute granules similar to oil granules, highly refractive; and sometimes, but not always, here and there a scale of epithelium, which, there is no doubt, becomes entangled in the fluid as it passes off, being furnished probably by the mucous surface about the orifice of the uterus. At least such scales do not properly belong to the true menstrual fluid, as it is occasionally observed to be entirely free from them.

"It may be proper to state that, of the several cases submitted to examination, no difference could be detected as to the leading characters of the fluid between one specimen and another, with one exception. This was a case of menorrhagia, or morbidly profuse menstruation, wherein the discharge, part of which exuded from excoriated surfaces on the *labia uteri*, was as florid as arterial blood, and so thin, that it trickled in drops along the tube, although but slightly inclined, to its outer aperture. This patient possessed the lymphatic temperament, was very irritable, and of relaxed fibre.

"From the above-mentioned appearances I conclude, that the *true* menstrual blood is extremely like that circulating through the capillaries in most of its leading properties, probably in all. The circumstance of its ready coagulation is conclusive as to the presence of fibrin, but in what precise proportion I have had no means of ascertaining: it is probable, however, that the relative quantity of this constituent in the fluid under consideration, is not different from that which obtains in the circulating mass in its healthy condition.

"The following observations and experiments furnish additional arguments in favour of the near approximation, if not the identity, in their leading characters, of the systemic and the *true* menstrual blood as it issues from the parts which secrete it; and tend strongly to prove, moreover, that the properties which are said to characterize *common* menstrual blood—namely, its want of power to clot, attributed to the absence of fibrin, its acid reaction, and its peculiar colour and consistence—are altogether acquired, and variously modified, by admixture with the vaginal mucus after the fluid has escaped from the uterine vessels.

"I have met with a considerable number of instances of what appeared to be natural menstruation during pregnancy and the nursing period, wherein the crude fluid collected from the *ostium externum* was precisely similar, as believed by the patient herself, and so far as I was able to ascertain by careful examination, to that discharged under normal circumstances. On examining these cases with the speculum during the existence of the menstrual phenomena, the blood was invariably found issuing from diseased surfaces situated upon or about the *labia uteri*, none escaping from the interior of the organ.

"I have also met with a number of cases of pseudo-pregnancy, the enlargement depending upon a faulty circulation through the vessels of the uterine system, and consequent hypertrophy of these and the adjacent organs. The patients, however, believed themselves with child, having the abdomen enlarged, the mammæ developed, and experiencing what they considered to be the fœtal movements; but in whom menstruation was repeated at regular or irregular intervals. In these cases also, which, as well as those mentioned in the preceding paragraph, were generally attended with a form of leucorrhœa to be described in another chapter, the blood was seen oozing from ulcerated surfaces on the *labia*, the *os uteri* being closed and for the most part contributing no portion of the sanguineous product. In both these forms of spurious, or what may be termed vicarious menstruation, the blood could not of course be considered as of the true catamenial character, yet it possessed subsequently all the sensible properties of the ordinary menstrual blood on its arrival at the *os externum*.

"If blood that has been drawn from the basilic or some other vein, or collected from a sacrified surface, be allowed to flow into a cup containing pure or diluted acetic acid—the same acid which exists in a free state in the vaginal mucus—in the proportion of one part of pure acid to sixty parts of blood, and if the acid be properly diffused through the blood; the latter will remain uncoagulated, and become of a darker colour if the blood be arterial, but lighter if venous, and no traces of uncombined fibrin can then be found in it. These experiments have been over and over repeated with the same results. But the following may perhaps be considered more in point.

"If healthy systemic blood be allowed to flow into a cup containing a small quantity of healthy vaginal mucus, and the two be gently mixed together, the blood will remain uncoagulated, and, if the mucus be in sufficient quantity, no trace of uncombined fibrin can be detected in it. This conclusion was arrived at after the following experiments. 1st, Twenty drops of blood issuing from a scarified surface were allowed to fall upon the surface of a piece of glass, the temperature of which had been raised to that of blood.

About seven grains of healthy vaginal mucus were then incorporated with the blood, the operation of mixing being accomplished as gently as it was possible. The mass appeared of the same colour as that of the blood before mixing. It did not separate, but remained a homogeneous fluid for an hour or two, when it became spongy, and subsequently dried, but gave no appearance whatever of separation. It had a slightly acid reaction. 2dly, About ten grains of healthy vaginal mucus (collected by means of the speculum), were introduced into a small cup, into which a stream of venous blood was directed to the amount of ten drams. The fluids were slightly stirred together by means of a skewer, to which, however, no fibrin adhered. The blood became a little lighter in colour after mixing. At the end of six hours it was a soft spongy mass, without a trace of separation. After twenty-four hours, it still remained a soft spongy coagulum, but without any appearance of serum. It was decidedly alkaline. The blood was evidently in too great proportion in this experiment, a circumstance which it was attempted to obviate in subsequent examinations. It was suggested, also, that the agitation used in mixing the two fluids might operate in hindering coagulation: this contingency is also provided for in the following experiments. 3dly, Two small cups were provided of equal sizes. Into one were introduced twelve grains of healthy vaginal mucus; the other was empty. Four drachms of venous blood were then allowed to flow into each cup, and both were agitated the same length of time, one for the purpose of mixing the fluids, the other with a view of ascertaining how far coagulation was interfered with by such operation. It was found, however, that, from some cause, perfect mixture of the mucus and blood could not be accomplished; small gray isolated portions of the former being visible in several parts of the fluid after the attempt to combine them had been made. The blood in each cup exhibited alkaline properties in an apparently equal degree of intensity. In eight hours, the unmixed blood was perfectly separated into a softish-looking coagulum and a clear serum, which completely surrounded and covered the clot. That in the other cup had become a spongy mass, but showed no signs of separation. On the following day, the separation of the unmixed blood was still more complete, and two or three drops of a transparent oily fluid appeared on the surface of the other, but could not be poured off from the clot. 4thly, The first experiment was again repeated, with the precaution adopted in the third, respecting the agitation of a similar quantity of unmixed blood. The results were precisely the same in the case of the mixed fluid. It remained perfectly uncoagulated, and was every way similar to ordinary menstrual blood: it exhibited an acid reaction. The other specimen was perfectly coagulated in from fifteen to twenty minutes, and at the end of an hour was quite separated. The relative proportions of mucus and blood used in this and the first experiments, I apprehend to be very nearly those of which the natural fluid is composed under normal conditions.

"It appears, then, from what has now been stated, that the true menstrual blood, uncombined with any other product, is extremely like the circulating mass, consisting of the same elemental constituents, and observing the same habits under like circumstances. And, escaping from the uterus in the separated form before described, the small clots are immediately dissolved in the vaginal mucus, being thus enabled to pass off in an uninterrupted stream; the fluid at this period possessing properties very different from those of either of the products of which it is compounded."—Pp. 21–27.

Here then is the explanation given by the author, of the peculiar character of the blood of the menstrual secretion. When discharged from the vessels of the uterus, it is quite similar to blood circulating in the blood-vessels. But as it is mixed with the vaginal mucus, the fibrinous portion is softened and dissolved; and it then presents itself as blood which does not coagulate.

The exact source of menstruation he establishes more accurately than has been done by any previous observer:—

"There is no doubt that the menstrual blood, under normal circumstances, proceeds entirely from the inner surface of the uterus, and that every part of the organ, including the upper portion of the *cervix*, is perhaps equally engaged in the performance of the function. This proposition is substantiated by the following case, showing the appearance of the uterus and its appendices of a virgin, who died at the age of seventeen years and five months, from the exhaustion consequent upon menorrhagic menstruation."—P. 32.

"An early post-mortem examination discovered no organic lesion in any part of the body, which was everywhere drained of blood. The uterus was rather larger than natural; its parietes were less firm, but nearly of the usual thickness. Its interior contained a clot of blood, which occupied the entire cavity. This clot, which was an exact mould of the uterine cavity, measured, from its lower extremity, which terminated at the *os tincæ*, to the part situated at the *fundus uteri*, two inches and a quarter, and, between its two horns, one inch and three-quarters. Each horn had a rounded extremity, was semi-transparent and fibrinous, and terminated at the corresponding Fallopian orifice. The Fallopian tubes were perfectly empty, and of the natural dimensions. The right ovarium was a little larger than the left, and presented several distinct cicatrices in different stages of reparation. One of these was recent, depressed, puckered, and appeared at first sight to have an opening in its centre; but it was afterwards found to be impervious. The cavity over which this was situated was occupied by a firm, reddish coagulum, traversed by yellow striæ, having an irregularly concentrical arrangement. Immediately beneath the next most distinct cicatrix was a firm yellow body, smaller in size than the preceding, and less striated. Another paler formation, of a similar kind, but still smaller and more deeply seated, and marked by a faint indentation of the surface above it, was observed in the same ovary. There were also two vesicles of different sizes, containing a thinnish glairy fluid, one of which was near the surface.

"The left ovarium presented on one of its sides a large bluish vesicle, projecting its peritoneal covering considerably above the surrounding surface; this was the size of a small hazel-nut, and contained apparently nothing but serum. Two smaller vesicles more deeply seated, and two well-marked yellow bodies of different dimensions having corresponding cicatrices, were also observed in the same ovary.

"The *labia* and *cervix uteri* were perfectly healthy. The inner surface of the uterus presented numerous openings scattered over every part of it, obvious to the naked view, some being sufficiently large to admit a good-sized bristle, or the end of a lachrymal probe. The largest and most numerous were at each side of the fundus near the horns of the uterus, and at the contracted part of its body near the commencement of the cervix. The openings had a valvular arrangement, a great number passing downwards towards the cervix, while those at the upper part of the organ appeared to pass towards the Fallopian orifices.

"The preceding case is interesting in a physiological point of view,—1st, In indicating the precise part of the uterus whence the discharge issued, and inferentially also the part which commonly furnishes the catamenial product; there being no doubt that the blood, the loss of which occasioned the death of the patient, escaped entirely from the openings now noticed; and that the hemorrhage, from beginning to end, was merely an exaggerated menstrual discharge, passing away, when in considerable abundance, in the clotted form; but being occasionally in smaller quantity, at which times it was not clotted, but assumed the general characters of the normal menstrual fluid. 2dly, The appearances described seem to suggest the *manner* in which the menstrual product is separated ordinarily from the circulating current; namely, by simple exudation from the arterial capillaries in communication with the valvular orifices naturally existing upon the inner surface of the uterus. The question of exudation, as applied to the manner in which the catamenial product is separated from the blood, in contradistinction to secretion (by which is meant the elaboration of a product different in character from that which furnished it), is strengthened by the appearance which the inner surface of the uterus is said to exhibit in persons who have died during menstruation; small portions of grumous blood being found attached here and there, more of which may be made to ooze out by pressure, making its escape from numerous vascular openings interspersed over the whole extent of the lining membrane."—Pp. 34—36.

The author then enters on an important inquiry as to the influence of various external physical circumstances and other conditions on the period of puberty, as indicated by the establishment of the menstrual secretion; the influence of climate, external temperature, occupation, mode of life, national habits, tribual habits, family peculiarities, and similar circumstances. But as upon this subject the elaborate papers of Mr Roberton have already communicated all the most valuable information in the pages of this Journal, it would only involve repetition were we to enter at length into the author's details. We merely add one concluding observation.

> " But the results obtained by Mr Roberton respecting the age of the first menstrual crisis in this country, and those recorded by myself in the preceding tables, exhibit a difference of about nine months. This disparity I can account for only on the supposition that his inquiries may possibly include a greater proportion of educated females than mine; the menstrual advent taking place, as was already shown, considerably earlier in these than among the labouring and uneducated classes. It appears, also, from Mr R.'s report, that the per-centage of instances of puberty in this climate, occurring before the age of eleven years, amounted to 3·15, while those happening in a corresponding period, in my own records, do not exceed 0.225 per cent."—P. 98.

The disorders incident to the menstrual function all possess more or less influence in the capacity of the uterus for being impregnated. These diseases are considered in the third chapter, under the several heads of retention, suppression, difficult menstruation, vicarious menstruation, and hæmorrhagic menstruation.

In the fourth chapter is considered the last menstrual crisis, or the termination of that periodical visitation, and the phenomena with which the cessation is usually attended. Under this head are considered various morbid states, as purulent discharge, endo-uteritis, or internal uterine inflammation, disease of the cervix, and corroding ulcer.

The fifth chapter is devoted to the signs of pregnancy, of which a very ample and correct history is given.

In the sixth chapter Mr Whitehead enters on the subject of abortion, by giving a large proportion of statistical information on that accident.

The first object is to show the actual duration of the child-bearing period. This, it is generally understood, bears an intimate relation to the activity and presence of the menstrual functions. Females begin to menstruate about the age of fifteen and a-half, and contiune on an average to do so to the age of forty-seven years and a-half; and hence the duration of the child-bearing period in this climate is calculated at about thirty-two years. Mr Whitehead, however, justly infers that the child-bearing aptitude or faculty does not really exist during the last five or six years of this term; and in proof of this he gives a table of last pregnancies as they took place in thirty-eight females, in whom the facts were ascertained with very considerable correctness.

From this table the following results proceed:—

"The sum of the ages of the individuals recorded in the table, at the time of their last delivery, is 1586, giving an average of 41.73 years; the average age of the same individuals, at the time of their last menstruation, is 47.54 years; so that a period of nearly six years is here indicated, during which, although the menstrual function continued to be more or less efficiently discharged, and the health good, aptitude for procreation did not exist. They were all placed under equally favourable circumstances for the continuance of child-bearing, so far as regarded their matrimonial position."

This result is, it appears, not very remote from what has been generally believed as to the termination of the child-bearing period in the majority of females.

"A like period of uterine quiescence is observed before child-bearing begins. This, however, is not so much owing to want of fitness in the organs of generation to accomplish the end for which they are naturally designed; but rather to the custom which prevails among the higher and middle classes of society in this country, of discountenancing, as far as is practicable, early matrimonial alliances. It would be well if this wholesome example were more generally encouraged, or else controlled by some suitable interposition of the legislature."

It is manifest that this is a circumstance, not of physiological character, but of moral and social origin. Independent of this circumstance, we believe that it does not always follow that because menstruation is established, impregnation will be the necessary result either of marriage or of sexual intercourse. To fix the point, however, the author gives a table of the commencement of child-bearing as presented by 541 married women, natives of this country, in whom conception took place on the average about two months and a-half after marriage. The average period of the duration of pregnancy at the time inquiry was made, was about the end of the sixth month.

"In reference to the commencement of child-bearing, the following table exhibits the respective ages at which the first pregnancy occurred in 541 married women, natives of this country: conception having taken place, on the average, about two months and a half after marriage. The process was differently advanced in different individuals; the average period of the process at the time of the inquiry was about the end of the sixth month:—

TABLE VII.—*Age and commencement of child-bearing.*

No. of Cases.	Age at time of first pregnancy.	No. of Cases.	Age at time of first pregnancy.
1	15½ years.	16	26 years.
9	16 years.	12	27 years.
14	17 years.	6	28 years.
33	18 years.	8	29 years.
64	19 years.	9	30 years.
76	20 years.	5	31 years.
85	21 years.	7	32 years.
76	22 years.	2	33 years.
41	23 years.	2	34 years.
48	24 years.	1	37 years.
25	25 years.	1	40 years

> "The mean age of the individuals indicated in the preceding table, at the time the inquiry was instituted, was 22 years, or thereabouts; from this may be deducted half a year for the term to which the process had then arrived, leaving 21½ years as the average age at which child-bearing commenced.
>
> "It thus appears that the actual duration of the child-bearing period in the female of this climate, is about 20 years, namely, from the age of 21½ to that of 41½; and that a wife, placed under favourable circumstances, and in the enjoyment of health, may be expected to continue bearing children, at certain intervals, during the whole of this period."—Pp. 241—245.

In the table are seen few instances of marriage at a very early age. Thus, among the whole 541, one person only is pregnant at fifteen years and a half, nine persons at sixteen years, fourteen at seventeen years, thirty-three at eighteen years, and sixty-four at nineteen years. The majority is between nineteen and twenty-five years, the last inclusive, which embraces 351 females. After twenty-seven years of age the numbers are small, being six, eight, nine, five, seven, two, and one.

It appears, however, from the observations of Mr Whitehead, that early marriages are one of the curses of the working population, principally the Irish immigrants, of Manchester, as they are wherever that reckless race are settled.

> "It is truly lamentable," he states, "to witness the immense amount of wretchedness exhibited in the objects seeking relief at our public institutions, resulting from this circumstance alone. Girls often become wives while they are yet but children, not only in years and in intellect, but absolutely before the change of pubescence has been accomplished; and, in some instances, before its near approach is announced by the usual physical indications."

The effects of this habit, and the domestic condition of these people, are described in the following terms:—

"The preposterous custom alluded to in the text—the pernicious tendency of which is sufficiently obvious—is not the result of the factory, or of any other system peculiar to Manchester and towns of similar character, as some writers have alleged; it prevails principally among the poor Irish residents, who form so large a proportion of the population of manufacturing districts. Marriages in the fifteenth and sixteenth years of age are constantly taking place amongst them; not suddenly, and by stealth, as might be supposed, but openly, deliberately, with the previous knowledge, if not with the approbation of the parents, and under the sanction of a solemn religious ceremony.

"The future prospects of these 'candidates for happiness,' and of the generation immediately forthcoming, may be faintly shadowed out by taking a view of the state of moral and religious culture to which they have arrived at this important era of their lives. They have been trained up under the guidance of parents whose early lot was similarly cast with their own; and who, by the time their first-born is thus launched upon the world—not put out to learn a business, but married—have attained to the decent and discreet age of thirty to thirty-five years, having already a family of from six to ten or twelve children.

"In nine cases, at least, out of every ten, neither party can read or write. The juvenile husband, if fortunate enough to be in employ, holds a precarious situation by which he probably earns from six to ten shillings per week, and the wife realizes perhaps six or eight shillings more. But want of employment does not deter them from entering into the bonds of wedlock, provided they can obtain sufficient money to pay the marriage fees. If employed, however, the young couple live in comparative luxury for some weeks, or months, until by sickness or other misfortune, or the contingencies of child-bearing, their supplies are cut off, and thus they become reduced at once to a state of pauperism.

"Their household expenses are very trifling, and these discharged, the rest of their earnings is spent upon articles of daily consumption. Provision for the following day, or even for the following hour, never seems to form the subject of thought. They are content to lodge in the same cellar or garret with their parents and family, or with other parties; they share the same litter of straw or of shavings with other couples similarly circumstanced; they participate in the same domestic conveniences, the extent of which is somewhat singular and interesting. No furniture adorns the apartment where they live, beyond one or two chairs or stools, and a sort of table. There is a corresponding supply of culinary utensils: one or two knives and forks, one or two spoons;—for the purpose of glossological investigation, I have frequently been unable to procure a fragment of either, except from the house of a neighbour;—

there is a very scanty supply of earthenware; and a single tin vessel is made to serve many purposes, besides that of cooking-pan.

"In many instances, the floor of the room is never washed, or even swept; the fire-place is never cleansed; and the ashes remain unremoved, except when the fire will no longer burn. The walls are never white-washed. Some time ago I made particular inquiries upon this subject, of parties occupying several streets of houses in a district known by a name which indicates the origin of a great proportion of its inhabitants. Amongst them were individuals who had resided upon the property for a period of from twelve to twenty years, and not one could be found who remembered any of the houses being either whitewashed or painted. The walls and ceilings were worse than black, and stained by thousands of marks of slaughtered vermin, which had been crushed with the finger, or pursued with the flame of a candle, as they crawled along upon the mortar. The stench in most of these places was pestilential and suffocating. A pig not unfrequently occupies a corner of the apartment where the inmates eat, and sleep, and cook their victuals.

"The individual condition of these people is much in keeping with that of the places they inhabit. They are extremely inattentive to personal cleanliness. They seldom properly cleanse their skin, and perhaps never extend the process beyond the part immediately exposed to view. Their linen is worn for an indefinite period without changing or washing. I have frequently witnessed a garment worn, unchanged, until it has fallen from the person in tatters. During the disasters of sickness, and especially during the puerperal period, the articles of dress nearest the skin become absolutely putrid. The whole drapery about the bed is literally saturated with filth, and alive with vermin, which are suffered to crawl unmolested in numbers beneath the eye; and the emanations are overpowering.

"In the economy of house-keeping their deficiency is most humiliating. What knowledge, indeed, can be expected to be possessed by a creature who has been employed from home since the age of eight or nine years, for twelve or fourteen hours daily, and has never even witnessed any of the processes of cooking beyond that of brewing a cup of tea, or boiling a potatoe. Their diet consists principally of dried fish, potatoes, and bread, and of expensive (comparatively expensive) ready-cooked articles procured from the shops. The meal is never set forth in order upon a table; but distributed immediately from the pan or the smouldering ashes, as it approaches a state of edibility; his share of the morsel being seized by each and consumed after his own fashion. Of the idea of 'family circle' and 'fire-side enjoyment,' they are utterly unconscious. The meal, such as it is, being finished, an adjournment to the dram-shop is a very common practice; and the intoxicating liquor is often poured down the throats of children five or six years old.

"They have no idea of the use of the loaf except while it is new, regarding it as useless or fit only for the pig when stale. Pieces of the finest bread, consequently, are often seen scattered about the sick room in quantity sufficient to furnish one of the most suitable articles of diet that the invalid could desire, if only a little trouble were bestowed in preparing it. Arrow-root, sago, and other nutritrious articles are occasionally supplied from some of the medical institutions; but in general, they lie neglected and untouched, and are ultimately thrown away for want of knowing how to prepare them for use. Yet is a demand being constantly made by the patient, or those about her, for something nourishing; referring always to wine or spirituous liquors.

"Those who are in the habit of coming frequently in contact with the lower orders of the Irish peasantry, cannot avoid the conviction that the origin of a vast proportion of the misery which prevails amongst them, both in this and their own country, may be directly referred to these very imprudent and unnaturally early marriages; to say nothing of the numerous instances of physical imperfection and mental decrepitude witnessed in the offspring of such unions." —Pp. 241—244.

For this state of matters it seems difficult, perhaps impossible, to apply any remedy. This condition is evidently one leading cause of the sickness and mortality among their children, so often noticed in the pages of this Journal; and in that point of view it deserves consideration and probably interference of some kind. But it seems difficult for the legislature to interfere in any other way than indirectly, by endeavouring to instruct them and teach them the necessity of more prudent and less hasty proceedings? But what legislature or government is adequate to this undertaking? It seems as if the great majority of the Irish were either incapable of being taught what is proper and for their own interest, or were to be always like grown children in the hands of their religious advisers. An evil of still greater magnitude is that their example in all large towns contaminates the natives of the place, and often makes them abandon their old habits, to adopt those of their Irish neighbours.

The author having fixed the period of child-bearing at the space of twenty years, next considers the number of pregnancies which may take place with each married female during that time. On this point he gives the following statements:—

"The number of pregnancies which each woman experiences, under the conditions stated in the preceding section, during the existence of her procreative aptitude, is about twelve, or one in every twenty months. This includes abortions, 'false conceptions,' so called, premature deliveries, and all having an unsuccessful issue, the average amount of which will be rather more than one and a half for each individual; or it may be stated as a general rule, that every seventh pregnancy has a premature termination. These conclusions have been drawn from the subjoined facts.

"Two thousand married women in a state of pregnancy, admitted for treatment at the Manchester Lying-in Hospital during parts of the years 1845 and 1846, were interrogated in rotation respecting their existing condition and previous history. Their average age, at the time of the inquiry, was a small fraction below 30 years. The sum of their pregnancies, already terminated, was 8681, or 4.38 for each; of which, rather less than one in seven had terminated abortively. But as abortion occurs somewhat more frequently during the latter than in the first half of the child-bearing period, the real average will, consequently, be rather more than one in seven.

"Of the individuals submitted to inquiry, 1253 had not then suffered abortion. The average age of these was 28.62 years; the sum of their pregnancies was 3906, or 3·11 for each person.

"The remaining 747 had already aborted once at least, some oftener. Their average age was 32.08 years. The sum of their pregnancies was 4775, or 6·37; that of their abortions 1222, or 1.63 for each person.

"With a view of ascertaining what degree of uniformity was observed in the occurrence of these events at different periods of the inquiry, the whole were arranged in ten equal groups, occupying for their registration about ten months in succession. It may be stated that the number now produced does not include all the patients admitted for treatment at the institution in question during the period alluded to, being only about two-thirds of the usual amount in a given time; for a considerable number appeared to remember so very imperfectly the events of their previous life, that their accounts were judged altogether inadmissible in a record of this character.

TABLE VIII.—*Giving an Abstract of the histories of* 2000 *pregnant women in reference to their age, the number of pregnancies, and proportion of abortions already completed; arranged in groups of* 200 *each.*

	Women who had not aborted.				Women who had already aborted.					
	No.	Average age when admitted.	Sum of previous pregnancies.	No. of pregnancies for each person.	No.	Average age when admitted.	Sum of previous pregnancies.	No. of pregnancies for each person.	Sum of their abortions.	Average No. of abortions for each person.
1st 200	129	28·83 yrs.	321	2·48	71	30·71 yrs.	445	6·26	110	1·55
2d 200	129	27·84 yrs.	376	2·91	71	31·92 yrs.	447	6·30	132	1·85
3d 200	120	28·04 yrs.	377	3·14	80	32·38 yrs.	536	6·70	134	1·67
4th 200	135	28·71 yrs.	414	3·06	65	32·77 yrs.	441	6·78	106	1·63
5th 200	122	27·63 yrs.	390	3·20	78	31·43 yrs.	448	6·00	127	1·62
6th 200	129	28·65 yrs.	423	3·28	71	32·29 yrs.	413	5·81	107	1·50
7th 200	114	29·95 yrs.	471	4·13	86	31·51 yrs.	554	6·44	149	1·73
8th 200	119	29·07 yrs.	429	3·60	81	32·03 yrs.	517	6·38	116	1·55
9th 200	130	29·20 yrs.	331	2·54	70	32·88 yrs.	476	6·80	111	1·58
10th 200	136	28·29 yrs.	374	2·96	74	32·87 yrs.	481	6·50	130	1·75
Tot. 2000	1253	28·62 yrs.	3906	3.11	747	32·07 yrs.	4777	6.3	1222	1·63

"From the preceding statements, it appears that more than thirty-seven out of every hundred mothers experience abortion before they attain the age of thirty years. This is, probably, rather under than above the real average, as a considerable proportion of them were the first time pregnant, having as yet all the difficulties and dangers of this eventful period of life to encounter; and all were, of course, liable to further similar misfortunes, as it was impossible to declare that the present should be the last pregnancy, even in those most advanced in years; or that the term in the existing instance should be fully accomplished. As the age, therefore, at which the above averages were made, is a year and a half below the *middle* of the child-bearing period (which will happen, as was already shown, at about 31½ years of age), and on the supposition also that the proportion of unfavourable cases is as great in the latter as in the first half of the term, it must be presumed that the per centage of 37 will be found to be somewhat less than half the number which represents the actual amount of women, living and enjoying all the necessary advantages, who experience abortion during some part of the period. This is rendered still more probable by the facts—too limited perhaps to warrant a general conclusion, but bearing directly upon the subject nevertheless—that of 64 women who were living in wedlock until after the final menstrual crisis, only 8 had escaped having an unsuccessful pregnancy; the per centage of those who had aborted being 87.

"It is popularly believed that early, especially first pregnancies, have more frequently a premature termination than those which come after. This does not accord with the results of my observations. I am inclined to believe that the third and fourth, and subsequent pregnancies, and one or two of the last—those, namely, which occur near the termination of the fruitful period—are most commonly unsuccessful. Two hundred and twenty-six of the individuals alluded to in the preceding table were the second time pregnant, of whom 20, or 8.8 per cent., had aborted of the first; and two hundred and thirty were the third time pregnant, of whom 58, or 25.20 per cent., had previously aborted."—Pp. 245—248.

Abortion may take place at any period during pregnancy. It is, however, more common at certain stages than at others, and is attended with different degrees of danger; variations which depend on the nature of the predisposing and exciting causes. When it takes place before the end of the sixth month, it is invariably fatal to the offspring, either before birth or a short time after. In the mother it is always dangerous, and sometimes fatal.

From a tabular view of six hundred and two cases of abortion, the following results may be given:—At two months there were 35 cases; at three months, 275 cases; at four months, 147 cases; at five months, 30; at six months, 32; at seven, 55; and at eight months, 28. Consequently, the greatest number of cases is at three months; the next at four;

and the smallest in the eighth month. In the seventh month they are more numerous than in any other, excepting third and fourth months.

Among these six hundred and two cases, 85 were still-born, 30 were living at birth, and only four were living at the end of one month. Of eight born alive at the sixth month, seven died within six hours after birth, and only one survived to ten days. Four hundred and eighty-three were in the state of dead or blighted ova.

In three of these cases the event was fatal to the mothers. One was a case of placental presentation, in which delivery was preceded by constant and profuse hemorrhage. One was an instance of malignant disease of the uterus. The third was an example of twins, at about seven months of uterine life.

In the seventh chapter the author enters on the subject of the causes of abortion, or premature detachment of the ovum. These causes he distinguishes, like most authors, into predisposing, and occasional or exciting. The immediate or determining cause, or that state of the womb and its contents which immediately precedes expulsion, he regards as common to the parturient process at all periods.

> " By *predisposing causes*, are meant certain morbid conditions, local or constitutional, already in the system; or a particular susceptibility to morbid action during pregnancy, by the operation of which the process is liable to be prematurely arrested. They have been subdivided into two orders: those, namely, which originate entirely in the maternal system; and those which appear to depend upon some defect in the product of gestation. Of the first kind are, diseased states of the uterus, including all the morbid conditions of which the organ is susceptible, as tubercular formations; polypous and fibrous growths; hydatids; dropsy; rigidity of the uterine fibres; abundant leucorrhœal discharges; dropsy of the amnion; accumulation of fluid between this membrane and the chorion; plurality of *ova;* and disease of the uterine appendages: also, certain constitutional conditions, as plethora; cachexy; morbid irritability; the hæmorrhagic, scorbutic, or scrofulous diathesis; and certain transmitted peculiarities. Some particular states of the atmosphere seem to create, for a time, an inaptitude for the accomplishment of utero-gestation; the more frequent occurrence of abortion, at some seasons than at others, giving it an epidemic character. This is undoubtedly the case in regard to some forms of disease incidental to the puerperal period, of which many remarkable examples might be furnished out of the events of the last few years."—Pp. 251, 252.

The catalogue now given is so extensive, and embraces so many particulars, that it may seem strange that abortion is not more frequent than it actually is. For if we consider the chances of disease, either in the system at large, or in the organs of reproduction, we find that numerous causes conspire against the life of the ovum being continued to the full time.

It further appears, that diseased states of the organs of reproduction are much more common than has been hitherto usually believed, and that they come in for a large share of influence in destroying the ovum, or causing its premature detachment.

In a former number* we had occasion, in speaking of the treatise of Dr James Henry Bennet, to direct attention to the great frequency with which that gentleman represents the womb to be affected in its cervix and orifice, with inflammatory ulceration and other morbid states, to the effects of these morbid states, and especially to their influence in causing sterility and abortion. We believe that several persons were disposed to think that this alleged frequency of these diseases was overrated by Dr Bennet. The present author, however, adds his testimony, and it is by no means feeble or without great weight, to the evidence already adduced, to show that Dr Bennet has not in this matter overstated the facts or the inferences to be thence deduced.

> "The admirable treatise," says Mr Whitehead, "on 'Inflammation, Ulceration, and Induration of the Cervix Uteri,' recently presented to the public by Dr J. H. Bennet, will have tended materially to direct attention to this branch of medical science; and the subject will doubtless increase in interest as the spirit of inquiry shall become more generally awakened. The opinions promulgated by this author, however, are far from being universally adopted; many are willing to believe his accounts exaggerated, although I have nowhere heard that experience has proved them incorrect. My own personal knowledge enables me unhesitatingly to affirm, on the authority of a multitude of facts, that Dr B.'s statements in reference to the extreme prevalence of uterine disease, are not only borne out in practice, but as regards their occurence during pregnancy, to which department of the subject he has but incidentally alluded, they are decidedly underestimated."—Preface, pp. xiii. xiv.

Dr Bennet is rather disposed, we find, to be dissatisfied with this acknowledgment of the value of his researches, and is desirous to claim more merit than is here allowed him. While we willingly admit that the merit of Dr Bennet is very great, in directing attention to the frequent prevalence of utero-cervical disease, and that he distinctly states its influence in the functions of the uterus; we think that he ought to be satisfied with the fact, that the present work contains the most ample and indisputable evidence of the general accuracy of his doctrines; and, that without the appearance of that evidence as corroborative, these doctrines, however well founded, would have been still called in question and doubted by many. We further subjoin various propositions, embodying his leading principles, which Dr Bennet has since published, and to which he has called our attention:—

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* Edinburgh Medical and Surgical Journal, Vol. lxvi. p. 226. Edinburgh, 1846.

"1. Inflammatory ulceration of the uterine neck is not an uncommon disease in the gravid uterus, although hitherto entirely overlooked by uterine pathologists and by accoucheurs.

"2. When this disease exists in the pregnant state, its symptoms are the same as in the non-pregnant condition, but obscured, and more or less modified, by the pregnancy.

"3. It is a frequent cause of disordered health during pregnancy, or of 'laborious pregnancy.' It is also one of the most frequent causes of abortion, both in the early and in the later months of pregnancy. It may occasion abortion, either directly, by reflex action, or indirectly, by giving rise to disease of the ovum or placenta, or to uterine hemorrhage.

"4. The instrumental examination of females labouring under inflammatory ulceration of the cervix during pregnancy is unattended with any risk, either to the mother or to the fœtus; and it is absolutely necessary, in order not only fully to recognise the disease, but also to treat it.

"5. The treatment of these forms of uterine inflammation must be governed by nearly the same rules in the pregnant state as in the non-pregnant state. A properly-conducted treatment is nearly always successful in preserving the life of the child and the integrity of the pregnancy, as well as in curing the inflammatory and ulcerative disease. It is also the only means we possess of warding off the imminent danger of abortion to which the patient is exposed.

"6. This form of uterine inflammation being, generally speaking, the cause of those repeated and successive miscarriages which prevent females giving birth to a living child, it is only by curing it that we can hope to make them bear the product of conception to its full period.

"7. The serious inflammatory and hemorrhagic symptoms which sometimes follow abortions are generally occasioned by unrecognised inflammatory ulceration of the uterine neck. On investigation, it often becomes evident that this disease existed previous to the abortion, and occasioned it. The same remark may apply to some cases in which the above-mentioned symptoms precede and follow labour at the full time, as ulcerative inflammation of the cervix in the pregnant state is by no means *necessarily* followed by abortion.

"8. Although inflammatory ulceration of the cervix seems generally to be a cause of sterility, yet, as will appear from the essay referred to, there are frequent exceptions to the rule. In some females, the tendency to become impregnated is so great, that no amount of uterine disease appears to prevent conception taking place."

In observing and narrating the history of the progress of scientific knowledge, and the introduction of new and more correct views as to the causes of disease, nothing is more difficult than to award to each discoverer the just share of merit, to which he may be entitled. It has oftened happened, and will again often happen, that the same new views are made simultaneously, by independent and unconnected observers; and that while one more fortunate than others, is first to proclaim a new fact, or give a new view in explanation of a difficulty, or announce the discovery of a series of facts, others perhaps may have been arriving by a similar course of observation at the same results. It seems impossible to doubt, that in the present instance, Mr Whitehead has been for a long time, perhaps several years, gradually becoming familiar with the facts which form the foundations of the doctrines contained in the present volume; and while we concede to Dr Bennet the merit of being the first to proclaim, in this country, the doctrines referred to, yet a certain amount of merit is also due—that of corroborating and confirming the accuracy of these doctrines—to the author of this elaborate volume. This will appear from the statements now to be adduced, on the morbid conditions of the uterus and its appendages, which cause abortion :—

> "Disease or malformation of the *ovum* or its appendages, and morbid organic changes of the placenta, said frequently to exist as causes of abortion, are so constantly associated with disease of the maternal organs, as to lead to the suspicion that the mischief in a great majority, if not in all instances, originates in the latter. In my own practice, such complication has been almost invariably found in those cases wherein I have had an opportunity of making the necessary examination. I can state, without hesitation, that in five out of every six instances of what are denominated 'blighted ova,' disorganized placenta, and hypertrophy or other abnormal condition of the membranes, organic disease of the uterus has been met with: and this, on inquiry, was generally proved to have existed at a period anterior to that from which the defect discovered in the aborted organs could reasonably date its commencement. There appears to be no reason for doubting, that not only disease of the product of conception, but also arrest of development, and consequent organic deficiency or malformation, are the direct result of a faulty condition of the organ or of the constitution upon which its growth and existence depend, and not the effect of any inherent imperfection, or of merely fortuitous causes.
>
> "Among accidental causes of abortion, the most common are, violent mental emotion, as excessive grief, joy, fear, anxiety, anger,

and the like; falls, blows, bruises, severe efforts, sudden movements of the body, as running, dancing, jumping; hysteric convulsions, epileptic fits, or severe pain; immoderate laughter, crying, coughing, vomiting, dysentery, diarrhœa; the use of strong purgative, emmenagogue, or mercurial medicines; the shock of the shower-bath, the hot foot-bath, copious bleeding; tight lacing, or any other inconvenience arising from dress; and whatever has a tendency to disturb the equilibrium of the circulation, or derange the balance of the nervous system. Sometimes, however, injuries of the most serious description, received during pregnancy, are recovered from without in any way disturbing the process. In February, 1846, a poor woman in the eighth month of pregnancy, was admitted a home-patient of the Lying-in Hospital, having been recently discharged from the Royal Infirmary, where she had been nine weeks an inmate for fracture of the skull, caused by a blow from a hatchet which she received in a quarrel in the preceding November. She was delivered at the end of March, of a healthy child, at the full term of utero-gestation. Numerous cases of similar character are elsewhere on record.

" And although the action of cathartic, and what are commonly denominated emmenagogue medicines, may sometimes be followed by abortion, I believe such to be a very unusual occurrence, except some powerful predisposing cause was already prevailing. The most alarming symptoms are sometimes induced by the use of the bitter apple, savin, rue, fox-glove, and other drugs, administered with criminal intention, without in the least degree disturbing the function of the uterus during pregnancy. Even the ergot of rye, which has certainly appeared, on some occasions, to exert a powerful effect upon the contractile fibres of the uterus, has also so frequently failed when given with a particular view to this end, that its specific power is now, by many practitioners, considered doubtful. In a case under my care, of pregnancy in a woman with extreme deformity of the pelvis, wherein it was considered advisable to procure abortion in the fifth month of the process, the ergot alone was employed, and, at first, with the desired effect. It was given in three successive pregnancies, and in each instance labour pains came on after eight or ten doses had been administered, and expulsion was effected by the end of the third day. It was perseveringly tried in a fourth pregnancy in the same individual, and failed completely.

Causes upon which Abortion appeared to depend, in cases having occurred under my own observation.

I questioned 2000 women on their admission as patients of the Manchester Lying-in Hospital, as to their previous history in reference especially to their unsuccessful pregnancies, and the causes assigned by them and their medical attendants for the misfortune in each instance. Of this number, 747 had already aborted once at least; to some, the event had occurred several times. The sum of their abortions was 1222. The following are the causes to which they had been attributed:—

" Inward weakness," impaired state of the health generally, and acute disease	911
Accidents, mental perturbation, &c.	222
No assignable cause	90
	1222

" In interrogating poor, uneducated women, respecting their personal history, great caution must necessarily be exercised to avoid being led into error. They are so regardless of essential particulars, so liable to forget events as they really occurred, and have such an irresistible propensity to amplify upon any topic relating to their own sufferings, that their stetements must, at all times, be received with a degree of limitation. At the same time, the expressions which they commonly employ are often strikingly graphic and significant, and if allowed to flow freely, unlarded by attempts at refinement of speech, their meaning is clearly and forcibly conveyed in a few words. Thus, 'inward weakness,' which is sometimes also denominated a 'waste,' refers always to a light-coloured vaginal discharge unconnected with menstruation; and, doubtless, either of these terms conveys a far more comprehensive idea of the patient's feelings, and the impaired state of health with which the complaint in question is, for the most part, associated, than that afforded by the more technical phrases of *leucorrhœa, fluor albus*, or the *whites*. The poor people are also in the habit of noticing a distinct variety of the whites, known by the appearance of the discharge, which they emphatically designate the 'yellows,' correctly regarding it as a more serious form of the malady. But their reports alone were not trusted to in the statements which follow.

" On a former occasion, whilst employed upon a course of inquiry of the present character,* I was frequently struck with the constancy with which leucorrhœal affections existed previous to the occurrence of abortion, and that the discharge was invariably accompanied by a certain, well-marked train of local and constitutional disturbances, and sometimes with evidence of disease of specific character. On submitting these cases to specular examination, the source of the discharge and the cause of suffering appeared to be at once revealed; disease of the lower part of the uterus being found to exist in almost every instance. That this lesion of structure constitutes the true pathological seat of leucorrhœa and of all its associated phenomena, as well as a very frequent cause of disastrous events during pregnancy, is further corroborated by the beneficial effect of the treatment adopted, when this was especially directed to the uterine affection. The following cases of abortion, examined principally with a view to obtain a correct statistical average of the prevailing causes thereof, occurred in immediate succession; a careful investigation having been instituted in each instance.

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* "The inquiry here alluded to was extended to upwards of a thousand of the earlier cases forming the subjects of investigation concerning the history of menstruation; but, on account of the incomplete and unconnected state of that portion of the records which has especial reference to this part of the subject, they were judged unsuitable for statistical purposes, and were consequently set aside on commencing the present series."

TABLE X. *Causes of, and conditions associated with abortion, in* 378 *cases.*

Accidental agencies.	Placenta prævia.	Constipation of the bowels.	Retroversion of the uterus.	Incurable disease.	Vascular congestion.	Disease of the lower part of the uterus.	Obscure causes.
44	8	3	3	1	15	275	29

"The cases indicated in the first five divisions of the preceding table I shall dispose of as briefly as possible, and proceed immediately afterwards, to the consideration of those more especially connected with a diseased condition of the uterus.

"Abortion immediately ensuing upon disturbance of the process of utero-gestation from *accidental violence,* is seldom brought under the notice of the medical man for the purposes of treatment, unless the injury inflicted be severe, and of a complicated nature; delivery being usually affected shortly after the application of the cause, and recovery being left to nature and the patient's own management. The possibility of internal organic lesion resulting therefrom, and its injurious effects upon the health prospectively, or upon future pregnancies, is altogether disregarded. In some of the cases attributed to these agencies, the exciting cause was of a very trivial description, and far less in degree than many women are known to bear with impunity. All those above recorded under this head, however, were previously free from vaginal discharge of any kind, which symptom I have taken as being, more certainly than any other, indicative of structural disease of the uterus. A few, whose recovery was protracted, and for a length of time incomplete, had leucorrhœal discharge, after the lochia had ceased; in a number of these the uterus was examined with the speculum, of whom three had fissured ulceration of the labia, two had the cervix indurated, with excoriation of the surfaces, and in two others a slighter form of granulating sore was present. It is probable that, in some of these, the disease existed previous to the occurrence of abortion.

"The majority of the above cases happened in women whose occupation necessitated a peculiar kind of bodily exertion; many being assistants to spinners, who are constantly obliged to use straining efforts to reach forward over the advancing machine; others were power-loom weavers; and a number were hawkers and market women, who are in the habit of raising and carrying heavy burthens."—Pp. 251—257.

"I visited a lady on the 4th of February 1846, four months advanced in her fifth pregnancy. She was of a full habit of body, and had previously enjoyed good health. Half an hour before my arrival, she had been suddenly seized, after taking a hurried walk, with uterine hæmorrhage, attended by a sense of fulness and intermitting pains of the back similar to those of labour. The orifice of the uterus was closed; the labia soft and tumid, but not diseased; the cervix was elongated and unoccupied; and the body of the uterus large, expanded and heavy, communicating the sense of *ballottement* when raised upon the finger. The placental *souffle* was distinct, but the fœtal pulsation could not be heard in any part of the abdomen. Twelve ounces of blood were immediately abstracted from the arm, by means of which instantaneous relief was afforded, both the pains and the hæmorrhage ceasing simultaneously. She was ordered to keep the recumbent posture, and restricted to a farinaceous diet. Six grains of camphor combined with four grains of extract of hyoscyamus were given at bed-time.

"On the following day, she was free from hæmorrhage and pain; but a sense of fulness of the abdomen being still complained of, ten leeches were applied to the hypogastrium. The dose of camphor and hyoscyamus was repeated at intervals, and this had the effect of keeping the system perfectly tranquil; and on the third day, an aperient was found necessary. The uterine bleeding did not return after the first day of the treatment; and by continuing the remedies, and observing still the above-mentioned precautions respecting diet and the recumbent posture, recovery was complete in seven or eight days. She was delivered of a fine healthy child of full growth, on the 2d of July. About a fourth or a third part of the placenta was thin, shrivelled, and condensed, being smooth on both surfaces, and having its cellular arrangement obliterated. There can be no doubt that this portion of the organ, which protruded from the *os* at the beginning of labour, had been originally attached over the orifice, and that its removal therefrom took place on the occasion of the hæmorrhage, five months before delivery."—Pp. 258, 259.

"Some idea respecting the actual prevalence of leucorrhœal complaints during pregnancy, may be formed from the following statement of facts, resulting from 2000 inquiries. Of this number, 1116 had the whites at the time the inquiry was made, and a considerable number more had suffered under a similar ailment at some former period. In 936, or 83 per cent., the discharge bore undoubted evidence of the presence of pus, or of sanies, and in some instances it was more or less mixed with blood: of these, 544, or 58 per cent., had previously miscarried. The discharge in the remaining 180 was said to be perfectly colourless, communicating no stain to the linen. I ascertained subsequently, however, that in several of these cases, the patient's statement, in reference to this point, was incorrect; the characteristic stain either having escaped observation at the time, or else the nature of the affection had undergone some change during the interval which had elapsed since the first inquiry. Such cases, therefore, ought properly to belong to the first-named division, although they have been retained as originally put down: 31 of these had previously miscarried. The following is the order in which these cases occurred, the whole having been subdivided, for reasons before stated, into groups of 200 each.

Table XI.—*Showing the number of instances in which leucorrhœa was found existing in two thousand pregnant women; the relative proportion of cases wherein the discharge was simply mucous; those in which it exhibited purulent properties; and the number of abortions which happened under each condition respectively.*

	Average prevalence of leucorrhœa during pregnancy.				Prevalence of abortion under the two forms of leucorrhœa relatively			
	No. of cases in which leucorrhœa existed.	No. of abortions for which *no cause* could be assigned.	No. of individuals entirely free from noticeable vaginal discharges	No. of abortions happening from *specified causes.*	Mucous leucorrhœa.	No. of abortions for which *no cause* could be assigned.	Purulent lucorrhœa.	No. of abortions for which *no cause* could be assigned.
1st 200	106	55	94	18	13	3	93	52
2d 200	99	62	101	20	12	1	87	61
3d 200	103	52	97	17	19	1	84	51
4th 200	108	45	92	16	24	4	84	41
5th 200	119	59	81	15	32	8	87	51
6th 200	108	52	92	16	21	4	87	48
7th 200	119	71	81	14	12	4	107	67
8th 200	125	68	75	18	17	1	108	67
9th 200	120	53	80	18	20	1	100	52
10th 200	109	58	91	20	10	4	99	54
2000	1116	575	884	172	180	31	936	544

" Leucorrhœa is much less frequently met with in the unmarried than in the married woman. In the former, the discharge is seldom purulent or sanious, and, consequently, is not often the result of suppurative action. It is generally associated with difficult or disordered menstruation; coming on, in some instances, at the period of puberty; in others, appearing as a compensating evacuation in cases of suppression of the menses. It is sometimes observed to recur periodically during pregnancy, at periods corresponding to the natural catamenial term; being accompanied by a similar train of symptoms, continuing the same number of days, and observing the same free interval as was customary with the menstrual function under normal circumstances. Or, when the morbid discharge is constant, it may be augmented at certain intervals, at which times it is not unfrequently mixed with blood. Persons most liable to such discharges, are those whose constitutional powers have been enfeebled by chronic disease, fever, loss of rest, the use of improper or innutritious food, indolence, dissipation, and a multitude of causes of a similar nature. Some constitutions are evidently predisposed to leucorrhœal affections; those, for instance, of a scrofulous or scorbutic diathesis, and the offspring of parents who have suffered severely from venereal affections which have been imperfectly cured, or improperly treated.

" Married women generally first become aware of a preternatural vaginal discharge, a few weeks or a few months after marriage. In those of an excitable temperament, who have previously been subject to inordinate determination of blood towards the uterine system, the discharge early exhibits purulent properties; and should this take place to any considerable extent before impregnation shall have been effected, the accomplishment of this organic act may be over and over frustrated, and sometimes long delayed; or the condition of the parts may arrive at such a state of derangement as entirely to prevent it for a length of time; and, in a still more aggravated form, it is not an unfrequent cause of barrenness.

"The period at which uterine disease most commonly begins to be troublesome, however, is during the second and third months of pregnancy, when the vascular apparatus of the generative system has arrived at a state of active congestion, very similar to that which immediately precedes inflammation. At this period, the organ will be found to have encroached considerably upon the lower part of the vaginal canal, where, increase in its lateral dimensions, with a simultaneous change effected upon the surrounding parts, resulting in turgescence of their textures, contributes to maintain it in an unyielding position;—circumstances which should always be had in remembrance in the treatment of the diseases of pregnancy, and especially in regard to the share which the matrimonial congress frequently has at this particular juncture, in preventing a favourable issue. There can be little doubt that a great number of cases of uterine disease, attended with vaginal discharge, and frequently resulting in abortion, may be attributed to intemperate sexual intercourse during the early months of pregnancy. Of forty-five women pregnant for the first time, all suffering under leucorrhœal affections, twenty-eight had the discharge of a decidedly purulent character. I examined the uterus in twenty-five of these, and all, with but one exception, had ulcerative disease of the lower part of the uterus; in the exceptional case, the vaginal membrane was studded with warty excrescences which were known to have had a syphilitic origin. I examined several of those also, who declared the discharge to be colourless, and found suppurative disease to be equally prevalent in them. Of more than two thousand individuals labouring under leucorrhœal affections, in whom I have examined the uterus with the speculum, I have, with comparatively few exceptions, found the existence of structural lesion sufficient in degree to account for all the symptomatic phenomena. In the great majority of the cases, the cervix and labia uteri were the seat of disease; in some there was excoriation, erysipelas, high vascular congestion, or a warty state of the vagina; in others, the labia, free from ulceration, were thickened, with their inner margins callous and flabby; or else they were tense, and presenting a vivid redness around the orifice of the uterus; both which appearances indicate the existence of disease within the organ.

"It sometimes becomes a question of considerable interest, and of no little importance in married life, to determine whether leucorrhœal discharges in the female are capable of producing the assemblage of symptoms in the male, constituting the ordinary phenomena of blenorrhagia. If questioned upon this subject, I should have no hesitation to return an answer in the affirmative, in all cases where the discharge in the female exhibits decidedly purulent properties; having myself witnessed several incontrovertible instances of the kind. And if further proof were wanting to place the matter beyond dispute, it might be supplied from the numberless cases of similar discharges in infants, coming on a few hours or days after their birth, and being, unquestionably, the result of purulent inoculation received during their transit into the world. Besides a number of examples of purulent ophthalmia, and also of purulent inflammation of the vagina in the female, and of the internal surface of the prepuce and urethral orifice, in the male infant, in children whose mothers have owned the existence of a yellow vaginal discharge before delivery; I have recorded the particulars of more than thirty cases of purulent ophthalmia, independently of affections of similar character in other organs, as the nose, ear, or anus, in which I have had ocular proof of the existence of uterine ulceration in the mother, both before and after labour."—Pp. 284—288.

Hemorrhage, it is well known, variable in amount, takes place during pregnancy in different females. It is often an indication of abortion, or some morbid state which may terminate in that event. On this subject the author gives the following inferences, deduced from an extensive collection of facts:—

"1. That *menstruation during pregnancy* is, for the most part, perhaps always, associated with an abnormal condition, generally with ulcerative disease of the uterus; requiring at all times active remedial interference.

"2. That hæmorrhage during pregnancy is not necessarily associated with an altered relation of the parts within the uterus, and, by timely care, need not interfere with the integrity of the ovum.

"3. That menstruation, during the early periods of lactation, is not always normal menstruation, but that it is generally associated with morbid conditions which are amply adequate to the satisfactory explanation of the phenomenon; that secondary hæmorrhage is, in the majority of instances, not owing to imperfect contraction, or atony of the uterine fibres; and that the discharge very probably proceeds, under these circumstances, not from the inner surface of the uterus, but from diseased surfaces situated upon parts external to the cavity of the organ."—P. 332.

Abortion we have seen almost constantly depends on previous morbid states of the uterus; and it is perhaps impossible to say what morbid state is most commonly productive of this occurrence, or whether any one morbid condition is exempt from producing it. The following observations on two of these morbid states, namely, the fissured ulcer of the *cervix* and *os uteri*, and the morbid conditions induced by gonorrhœa deserve special attention:—

"1. That fissurated ulcer of the cervix uteri constitutes a powerful predisposing, as well as a very common exciting cause of abortion.

"2. That ulcerative disease of the cervix is a common cause of uterine hæmorrhage, both during pregnancy and after delivery.

"3, That the phenomena of *menstruation during pregnancy* may be periodically developed without the co-operation of that part of the organ which is naturally adapted to the discharge of this function under normal circumstances.

"4. That the product of uterine disease, in some conditions at least, possesses virulent properties.

"5. That caustic and other applications to the uterus during pregnancy may not only be practised with impunity,—due attention being paid to the state of the system,—but that, in many instances, they are absolutely necessary, as well to the safety and comfort of the mother, as the preservation and health of the ovum,

"It was intimated that abortion, connected with the fissurated form of ulcer, commonly happened about the middle periods of pregnancy; it appears, however, that these events observe a somewhat extended range. The following statement represents four stages of pregnancy at which abortion occurred, or was imminently threatened under these conditions, in fifty instances, the majority of which had an unfavourable termination. In some, miscarriage had previously occurred, and appeared again to be impending; and in others the threatening symptoms prevailed for the first time:—

9 cases happened at from 11 to 14 weeks.
17 „ „ 15 to 20 „
19 „ „ 21 to 30 „
5 „ „ 31 to 37 „ —Pp. 346, 347.

"The first change operated upon the uterus after gonorrhœal inoculation, consists in superficial inflammation of one or both labia at their most depending part, or at the boundary of the os and commencement of the internal cervix. The inflammation seems to affect principally the small mucous follicles with which the surface is closely studded—(probably not the Nabothean bodies already noticed). A small red patch is first perceived; sometimes there are two or three isolated spots which extend and soon run together, forming one patch, of variable size in different cases and in different stages of the complaint, and generally of irregular shape. On removing the thick secretion with which this is covered, the surface appears to consist of minute granules, equally dispersed over every part of it; the abrasion is bounded by a margin not very distinctly defined, running imperceptibly into the erysipelatous redness which surrounds the sore; this extends to some distance upon the cervix, the whole of which is more or less tumid, but not painful to the touch.

"In persons possessing the sanguine or bilious temperament, and of plethoric habit of body, the symptoms are often violent; the excoriation extending rapidly, on the one hand upon the cervix in an outward direction, and on the other towards its lining membrane, the part upon which the erosion appears, first assumes the form of an angry, irritable ulcer, throwing off a large quantity of pus, and there is a great tendency to inflammatory fever. If the infection be contracted during pregnancy, the process of utero-gestation is often endangered during the acute stage of the complaint, especially in those of a nervous and irritable frame, in whom hysterical symptoms are readily developed. The inflammatory action in gonorrhœa, is extremely liable to extend within the uterus, and is probably a frequent cause of chronic endo-uteritis—one of the most serious and troublesome conditions of which the womb is susceptible. After the acute symptoms have subsided, the local disturbance becomes less diffuse; the erosion assumes the character of a simple granulating sore, situated upon an inflamed, indurated crust, and having a defined margin, which is manifestly elevated above the general surface. In this state the disease may continue for many years; rendering the whole of each succeeding pregnancy a period of suffering and misery, in many instances occasioning abortion at different stages of the process; sometimes it effectually prevents the accomplishment of impregnation, and it is not an uncommon cause of dysmenorrhœa. The symptoms are always aggravated during pregnancy; the inflammation involving the deeper textures of the cervix, and resulting in interstitial deposit, and consequent induration. After delivery, whether at the full term or prematutely, the ulceration is often found to have assumed the fissured variety.

"It is my opinion that gonorrhœal affections in the female are very rarely perfectly cured. In a vast proportion of the cases of simple erosion, of the fissurated ulcer, as well as of inflammation of the deeper seated textures, met with in the course of these investigations, the origin of the complaint was attributed by the patient to the 'bad disorder.' Many of them asserted also, that they underwent a course of treatment at the time, similar to that adopted in the husband's case, and that their complaints were considered as having been perfectly cured. Nevertheless, they had ever since been troubled with the same kind of discharge that appeared for the first time at the period in question, together with the train of sympathetic disturbances commonly indicative of the presence of uterine disease. In some instances, the complaint had been contracted during the existing pregnancy; in others, the date of its commencement was several years anteriorly. One patient, forty-five years of age, labouring under disordered menstruation and purulent leucorrhœa, had been, from time to time, a patient of first one, then another of the hospitals, and frequently under the treatment of private practitioners for the 'whites,' during a period of more than twenty years. She contracted a gonorrhœa from her husband shortly after marriage in her twenty-second year, for which she was under treatment at that time three or four months; her complaint was considered as perfectly cured, although the yellow discharge did not cease, but was present ever afterwards, except during the menstrual terms. She miscarried of her first pregnancy before the period of quickening, and had subsequently several 'false conceptions,' which were not looked upon as pregnancies. In all, according to her own reckoning, she had had fourteen pregnancies, seven of which terminated at, or within, six or eight weeks of the full period. The children, in all of these instances, were born alive; but all died emaciated in infancy, several of them having had purulent ophthalmia. The rest were abortions, in the sixth month of pregnancy. I found the cervix uteri deeply fissured and indurated, the labia excoriated; and there was an abundant muco-purulent discharge, with great constitutional disturbance. She was more than five months under treatment, during which time the system was once brought under the influence of mercury, and she took, during the whole of this period, the compound decoction of sarsaparilla, combined with iodide of potassium. Her ultimate recovery was most satisfactory.

"There is no doubt that the scientific principles of treatment of gonorrhœal affections in women, are greatly misapprehended. This is the natural result of a mistaken idea respecting the seat of the disease. Writers of the highest authority, including Hunter, Cooper, Howship, Swediaur, as well as those of more recent date, having considered it as affecting the urethra, Cowper's glands, the vulva, and the external genitals principally; while few have directed attention to its effects upon the uterus. I have seen no small number of cases of this disease, and have almost invariably found the lower part of the uterus inflamed or ulcerated; where the urethra or the adjacent structures have been involved, the affection of these parts has been proved to be of secondary character, resulting from a transfer of the morbid product from the seat of the primary inoculation—the uterus. I invariably treat them, wherever convenient, with nitrate of silver applied through the speculum, of which three or four applications are generally sufficient to effect a cure."—Pp. 362—365.

These observations bear the internal evidence of having been founded on careful observation and extensive experience; and though our testimony to their accuracy is not requisite, yet it is proper to say, that we have in a much less extended sphere, had often occasion to arrive at the same conclusions.

We must not, however, dwell longer on this subject; and all that we shall now do is to direct attention to the observations of Mr Whitehead on the employment of mercurial preparations, as remedial means for curing several of the states of the uterus already noticed.

"The employment of mercury in syphilitic affections requires the exercise both of skilful judgment, as well as of the most wary attention during its operation. While, on the one hand, there exists not a more valuable remedy under proper management, there is not, on the other, a more dangerous weapon, when wielded by the hand of the ignorant or the unpractised. Some constitutions are highly susceptible of its influence, and upon these a few grains of the more active preparations will often be found sufficient to produce unpleasant consequences. This may occasionally be attributable to idiosyncrasy; but the lymphatic and nervous temperaments, and the strumous and cachectic habits of body, appear to be pre-eminently disposed to such manifestations. Nevertheless, mercury is not to be altogether dispensed with, even in debilitated constitutions, if the venereal taint shall have been undoubtedly proved to exist; some of its preparations indeed, combining as they do, in admirable fitness, both alterative and tonic properties, seem eminently adapted to these cases. Such, for instance, are the oxymuriate and the proto-iodide. Strong, plethoric constitutions on the contrary, especially those of the bilious temperament, sometimes resist the action of the most powerful mercurial preparations, for a great length of time, even when given in very large doses. In these cases the oxyde and submuriate will be found the most appropriate forms, as internal remedies, assisted by inunction, mercurial fumigation, and such other auxiliary means as the necessity of the case may require. When it becomes desirable to administer mercury in large doses, especially if the preparation used be the submuriate, it will often be found expedient to combine it with suitable doses of opium, with a view of preventing it from passing too speedily away by the bowels. Its action is considerably promoted also by antimonials, ipecacuana, blood-letting, the warm-bath and sudorifics. In all cases, the treatment by this means should be continued until the secretions become decidedly changed; an object which, generally speaking, can never be considered as fully and safely attained, until the effects of the remedy are perceptible in the mouth; although it is never necessary to produce salivation. After this end shall have been accomplished, the vegetable alteratives must be substituted, and their use continued for a considerable time.

"The remedy of this class now universally employed is sarsaparilla, administered, for the most part, in form of compound decoction. As at present produced, it is an elegant and agreeable medicine, much in fashion, and is often productive of beneficial results, if continued sufficiently long. But the good derived from the use of the preparation in question is probably due to the mezereon, guaiacum, and sassafras wood with which it is combined, and not to sarsaparilla. Withhold entirely this tasteless, inodorous, and, in my opinion, inert article, and the preparation has very nearly the same sensible properties, and precisely the same effect as an alterative remedy will be derived from it. This subject, in an economical point of view, is one deserving of serious consideration, especially in hospital practice; many hundreds of pounds being annually expended out of the funds of some institutions in the purchase of this article alone. I have been in the habit for a length of time of using the preparation without sarsaparilla, but introducing an increased proportion of the other ingredients, with more decided benefit than is commonly secured by the ordinary form of the preparation.

"But we possess, among our own indigenous productions, many valuable, although neglected remedies of this class, and which, if properly used, would in all likelihood be amply adequate to the requirements of cases arising in the climate in which we live. This accords with the antiquated adage in reference to the 'sufficiency of the soil for the needs of her offspring.' Of the remedies here alluded to, I need mention only the *Solanum Dulcamara*, the *Arctium Lappa*, and the *Rumex Hydrolapathum*. The latter I have used in a great number of cases, not only of secondary venereal affections, but also in other forms of disease, with the most satisfactory results; and it is one of the most speedy and effectual remedies which I have hitherto seen used, for the purpose of rectifying the hæmorrhagic and scorbutic diathesis. I now generally combine it with *Taraxacum* and sassafras wood. The effects are often perceived after a few large doses have been taken; but its use should be continued several weeks at least,—in some instances, two or three months; I have sometimes introduced it as a substitute for sarsaparilla in the compound decoction of that name. The efficacy of the two preparations relatively scarcely admits of comparison."—Pp. 387—389.

These observations, recommending the employment of indigenous remedies, are judicious. They accord, as we have formerly shown, with the experience of the continental practitioners, who employ almost constantly these indigenous remedies in those complaints, in which British physicians and surgeons prescribe the more costly, and certainly not more active, exotic.

We have entered fully into the enumeration of the contents of this work, because these contents give proof of great industry, and much attention to discover and explain truth, and contain a large amount of useful and precise information. The work deserves, we further add, and it ought to obtain, a careful and repeated perusal, not only from obstetrical practitioners, but from all who wish to understand the nature, forms, and influence of uterine diseases.

ART. IV.—1. *Observations on the Anatomy of the Lungs.* By THOMAS ADDISON, M. D., Physician to Guy's Hospital. Medico-Chirurgical Transactions, Vol. xxiv. p. 146. London, 1841.

2. *On the Ultimate Distribution of the Air-Passages and Air-Cells of the Lungs.* By WILLIAM ADDISON, Esq., F. L. S. Read 7th April 1842. Philosophical Transactions. London, 1842.

3. *Extrait d'un Memoire sur la Structure intime des Poumons*

dans l'homme et les Mammiferes, (lu a l'Academie des Sciences, 11 Juillet 1842). By J. M. Bourgery. Gazette Medicale, 1842, Tom. x. N. 20.

4. *On the Minute Structure of the Lungs, and on the Formation of Pulmonary Tubercle.* By George Rainey, Esq. M.R.C.S. Medico-Chirurgical Transactions, Vol. xxviii. p. 379. London, 1845.

In the twenty-first volume of this Journal we gave an account of the researches of Francis Daniel Reisseissen on the minute structure of the lungs. Since that time, these organs, especially in reference to the mode in which the bronchial tubes terminate, and the fact of the existence or non-existence of air-cells, have been investigated by several anatomists—chiefly by the aid of the microscope. The effect of these researches has been, in certain points to confirm, in others to modify, and in others to controvert, if not to render doubtful, the results obtained by Reisseissen; and it becomes requisite to notice shortly these inquiries, at once for the sake of showing the progress of anatomical knowledge, and for the purpose of placing before our readers a general view of the results obtained by different inquirers.

The lungs may be regarded as the ramifications and terminal ends of the bronchial tubes, pulmonary artery, and pulmonary veins, all united by filamentous-cellular tissue and enclosed within the *pleura*.

The filamentous-cellular tissue now mentioned forms with these enclosed textures the pulmonic substance, tissue, or parenchyma.

The main point, however, which it is important to know, in order to understand either the structure of the lungs, as explanatory of their morbid conditions, or the functions of these organs during health, is the mode in which the bronchial tubes are distributed and terminate.

Before directing attention to the researches of recent observers, it is proper to take a short retrospective view of the representation given of the pulmonic cells or vesicles, by various anatomical authorities, during the last century and a half.

Malpighi, who first studied the structure of the lungs with attention, maintained that the *bronchi* terminate in closed ends, slightly expanded into the form of spherical or globular *vesiculae (vesiculae orbiculares)*, which he in one passage compares to the cells of bee-hives.*

Malpighi afterwards states that these membranous *vesiculæ* appear to be formed from the termination of the *trachea*, which, ending at its extremity and sides in ampullular or dilated cavities, terminates from these in spaces and *vesiculae* irregular in size. Again he adds, the substance of the lungs consists of lobules almost infinite, invested in a proper membrane, and provided with common vessels attached to the divisions of the windpipe.

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* De Pulmonibus Epistola, I. J. Alphonso Borelli. Marcelli Malpighi, Opera Omnia. Londini, 1686. Folio. Tomus Secundus, p. 133 and 134.

Next to the lobules, he notices interstitial spaces, which he says are not empty, but are formed by membranes, parallel in some instances, angular in others, and which proceed both laterally from the surface of the lobules, and also from the internal substance of these lobules; and between these are distributed the small vessels issuing from the lobules.

By these interstices, Malpighi appears to understand the investing filamentous tissue of each lobule, and probably that enclosing the air-cells.

This account shows that he believed that these *vesiculae* communicate with each other. He thought it also probable that these *vesiculae* are continuations or processes from the inner membrane of the windpipe,—in other words, from the bronchial membrane.

Duverney maintained, in 1699, that these vesicles or cells communicate with each other; and Stephen Hales, who examined them in the calf by the microscope, represents them as little cubes or hexaedral figures, not spherical, and estimates their diameter at $\frac{1}{100}$ part of an inch.*

This idea of vesicles or small cells was adopted by James Keill, Cheselden, Winslow, and various systematic authors; and the idea of Duverney that they communicate, appears at the same time to have been added.

Dr James Keill evidently translated the description of Malpighi. It is possible that he may have also examined the cells for himself. But his expression is so remarkable, that it evinces a strong coincidence with that of Malpighi, and perhaps the views given by Willis.

"The substance of the lungs," says Dr Keill, "is composed of an infinite number of little lobes, of various figures and magnitudes; but their surfaces are so adapted to one another, as to leave but very few and small interstices. These lobes are disposed like so many bunches of grapes upon the sides of the *bronchi*. Each little lobe contains within its own proper membrane, an infinite number of small orbicular vesicles, which leave small interstices between them, which are full of small membranes, like those which tie the lobes together. The extremities of the branches of the windpipe open into the cavities of these vesicles, which are probably formed by its membranes; but the capillary blood-vessels are only spread upon the vesicles like a net, with frequent and large inosculations.[*]

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* Statical Essays, Vol. i. pp. 239 and 241. London, 1731.

Such is the description of the air-cells of the lungs given in that treatise which served as a guide and text-book in anatomy to almost all English physicians and surgeons, from the end of the seventeenth century to the middle of the eighteenth, when it began to be supplanted by the system of Cheselden. In many respects, the account of the air-cells, though short, differs not much from that given by the most recent observers; and if Keill says little minutely, he does not commit any serious error.

According to Cheselden, the lungs are each composed of very small cells, which are the extremities of the *aspera arteria* or *bronchos*. The figure of these cells is irregular, yet they are fitted to each other so as to have common sides, and leave no void space.[†]

The language of Winslow is more specific: "Each of this multitude of bronchial tubes," he says, "is enlarged at its extremity, and forms a small membranous cell called a *vesicula*. These cells or follicles are intimately united together in clusters." Afterwards, he states, that the ultimate branches of the bronchi become the foot-stalks of small lobules; that the lobules are angular, oblong, large, or narrow; that these foot-stalks send between the lobules other membranous foot-stalks, smaller, and very short, and which terminate in the bronchial *vesiculæ* and cells, and are their continuations.[‡]

Senac next maintained that the lungs consist of lobules; that each lobule consists of *vesiculae*, in each of which ends a bronchial tube; and that each *vesicula* consists of small polyedral cells, not more than the sixth part of one line in diameter. This description, Senac admits, was mostly derived from examining the lung of the calf.[§]

Helvetius, on the other hand, was of opinion, from various examinations and experiments, that the representation of round *vesiculae* or cells was entirely a fiction; that what Malpighi describes as air vesicles are the cells of the cellular tissue; and that the closed ends of the bronchial tubes are the last recipients of the atmospheric air.[||] Helvetius argued that it was impossible to see these *vesiculae* attached to the extremities of the bronchi, that the minute elevations produced by inflating the lung are caused by the pressure of the air against the external membrane of the lung;

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[*] The Anatomy of the Human Body Abridged, or a Short and Full View of all the Parts of the Body. By James Keill, M. D. The Fourth Edition with Additions. London, 1710. 12mo. Chapter III. Sect. vi. p. 154.

[†] Anatomy, Book iv. chap. vii. p. 173. Lond. 1784.

[‡] Exposition Anatomique de la Structure du Corps Humain. Par Jacques Benigne Winslow, de l'Academie R. des Sciences, &c. Paris, 1732. 4to. N. 104 et 136.

[§] Traité de la Structure du Cœur, de son Action et de ses Maladies. Par M. Senac, Medecin Consultant du Roi. Tome ii. A Paris, 1749. 4to. p. 232.

[||] Memoires de l'Academie des Sciences, 1718, p. 24–28.

and that the spaces called interstices by Malpighi, were formed by the cellular tissue of the lung, passing from the pleura around and between the lobules.

Haller, though inclined to the doctrine of Helvetius, inferred nevertheless, that considering all the circumstances and the phenomena of experiments and dissections in living or recently dead adult animals, each bronchial tube does not terminate in an individual cavity, but that there is in the human as in the reptile lung, a cellular arrangement, the imperfect chambers of which communicate freely with each other, until the investment of each lobule delays the passage of the air, and prevents it from proceeding from lobule to lobule.*

Of these observers, Helvetius and Senac speak in a manner rather confused and sometimes contradictory; and, though it seems singular, that any one accustomed to inquiries of this kind could confound the terminations of the bronchial tubes, whether forming cells or not, with the cells of the pulmonic cellular tissue, there is reason to believe that this was done by Dr Hales, and probably by Helvetius.

The doctrine of communicating air-cells was accordingly very generally taught by anatomists, and is distinctly presented by Soemmering, who may be taken as the representative of anatomical doctrine at the close of the 18th century, and for the first fifth of the 19th. According to Soemmering, the substance of the lung consists of small air-cells or *vesiculæ*. Several of these *vesiculæ* form a cluster (*acervulus*); several clusters compose small lobules; these unite into large lobules; and these by their union form the lobes of the lungs.

These cells appear to be round, polygonal, and irregular. When inflated they are about the eighth or the tenth part of one line in diameter; and they communicate with each other through the windpipe, in such manner that air blown into one cell easily passes from this into the bronchia, and by these penetrates into all the other cells of the lungs. The cells of neighbouring lobules, however, do not communicate with each other; and it is only the cells belonging to each cluster and lobule that thus communicate.†

If this description be understood literally, it implies that the alleged cells of the lungs do not communicate with each other by themselves, but only by the arrangement of the bronchial tubes terminating in them. From this it follows, that these air-cells are mere shut ends of the bronchial tubes.

In 1808 Reisseissen examined in various modes the terminal ends of the bronchial tubes, and arrived at the conclusion, that the lung possesses no arrangement like that described as air-cells, and that each lung consists of a series of successively tapering canals terminating in the closed ends of the bronchial tubes, not communicating with each other, however, but which, retaining the peculiar structure to their extremities, present the appearance of air-cells or air-vesicles.*

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* Elementa Physiologiæ, Lib. viii. Sectio. ii. § xxix. § xxx. Tom. iii. p. 189. Lausannæ, 1766.

† De Fabrica Corporis Humani. Tomus sextus, § xxi.

The essay of Reisseissen was not published till 1822; and it was only subsequent to that time that his statements became known or their correctness ascertained.

Magendie examined in 1821 the minute structure of the lungs by inflating and drying small portions; and concluded that there is a cellular or vesicular arrangement at the extremities of the bronchial tubes communicating with these tubes, and in which these tubes finally terminate; that these cells present no regular form, and appear to be void of membranous walls; that they are formed by the last divisions of the pulmonary artery, the *radiculæ* or roots of the pulmonary veins, and the numerous anastomoses of these vessels; that all the cells of one lobule communicate with each other, but not with those of contiguous lobules; and that these cells vary in size at different ages.‡

Reynaud, who examined the bronchial tubes with the view of ascertaining the accuracy of the representations of Reisseissen, arrives at the following conclusions.

Any given bronchial tube, when traced through the lung, is found to divide before and behind, and on each side into small branches; which again traced still further are found to be subdivided into branches still smaller than these. These divisions become shorter and shorter, and of smaller calibre, end in becoming rounded, as if they had formed at their side a number of small shut ends or depressions, and at length they terminate in an extremity shut and scarcely enlarged. This is shown by mercury poured into them and urged forward by pressure, when the mercury demonstrates, as it were, the last divisions of the diminutive bronchial tree,—and also by insufflation and dissection. These small tubes all terminate at right angles to the *pleura*, which allows the observer to recognize only the assemblage of their terminal sacs, and not the bronchial tubes from which they proceed.

He mentions, however, the following disposition also. A greater or smaller number of bronchial tubes larger than others do not end thus at the *pleura;* but having come near it, instead of terminating more or less rectangularly at the pleura, proceed beneath this membrane parallel to it, and terminate at the distance of two, three, four, or five lines from their point of emergence. The air which filled these tubes appeared like the mercury, in the instance above mentioned, to represent these small trees with perfect regularity, and to demonstrate their arborescent disposition to that point, when pressure more or less forcible was inadequate to urge the air onward, and where it was evident it had reached its last limits.

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* Franz Daniel Reisseissen über den Bau der Lungen. Berlin, 1822. Folio; and Edinburgh Medical and Surgical Journal, Vol. xxi. p. 444. Edinburgh, 1824.

‡ Memoire sur la structure du poumon de l'homme; sur les modifications qu' eprouve cette structure dans les divers age; et sur la premiere origine de la phthisie pulmonaire par M. Magendie. Journal de Physiologie, Tome i. p. 78. Paris, 1824.

These ends of bronchial tubes are about two lines from the *pleura*, or even less; and to demonstrate them fully, it is often requisite to open them the whole length, and urge a bristle through them, so as to perforate that membrane, or to make a small counter opening. In this manner the end of the bronchial tube is displayed, showing the membrane continuous from the upper part of the tube throughout. The only other circumstance is, that at two lines from the termination, it looks as if perforated by many small depressions, which it had not previously presented. Small apertures leading into small ultimate branches, are likewise seen more and more near to each other.*

The general correctness of this description is strongly confirmed by the phenomena of obliteration of the bronchial tubes.

Bourgery made known to the Academy first in 1836, and afterwards in 1842, the results of researches on the minute structure of the lungs, of which the following is a summary. To every minute lobule of the lung is sent one central bronchial tube, which proceeds to the peripheral basis of the lobule, and is distributed with progressive ramification to the terminal ends of the same. During this course, the central stem sends within the lobule alternately, radiating in all directions, subordinate shoots, which are to be considered as terminal branches of the proper air-tube tree, and which Bourgery denominates *ramified bronchial canals*. Beyond these commences, according to M. Bourgery, the capillary air-sucking system. Each ramified bronchial canal ends in a small, irregular, winding, elongated dilatation, which is sometimes two-lobuled or three-lobuled. These are bounded in their course and in their dilatations, by walls perforated in a sieve-like fashion by small orifices, by which the branched and ramified bronchial tree, as an inferent and efferent apparatus, is connected with those parts of the lungs which are to be viewed as the proper functional substance. This part forms a labyrinth of tubes expanded in three directions, which are distributed in a tortuous course along the windings of the vessels, observe a proportional diameter, and at their ends also, as in the lateral walls, communicate with each other by many orifices.

Dr Thomas Addison maintains the existence of a collection of cells, in which a filiform bronchial tube terminates.

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* Memoire sur l'Obliteration des Bronches; par A. G. Reynaud, D. M., &c. Memoires de l'Academie Royale de Medecine, T. iv. p. 115. Paris, 1835. 4to.

"The results," says Dr Addison, "of my investigations, for example, seem to prove almost beyond dispute;—1st, that the aerial cellular tissue of the lungs is made up of well-defined, rounded, or oval lobules, united to each other by interlobular cellular membrane, each lobule constituting a sort of distinct lung in miniature, having its own separate artery and vein; 2dly, that these lobules do not communicate directly with each other; 3dly, that they do not, as Reisseissen and others have supposed, consist of the globular extremities of as many bronchial tubes, but, on the contrary, as my friend Dr Hodgkin has suggested, are made up of a collection of cells, in which, by a common opening, a minute filiform bronchial tube abruptly terminates; 4thly, that the pulmonary artery accompanies the bronchi, branch for branch, to the minutest divisions of the latter; 5thly, that pneumonia consists essentially in inflammation of the aerial cells."—P. 148.

In order, however, to form a just opinion of the views of Dr Addison, it is necessary to advert to the facts which he has adduced, in order to elucidate the mode in which the pulmonary vein is distributed.

"In order to accomplish this demonstration, the pulmonary artery was injected with size, coloured red, whilst the vein was injected with the same material, coloured yellow: the lung was then laid aside and kept moistened in a cool place for several days, with the view of softening, by approaching decomposition, the connecting cellular membrane distributed throughout the lungs. In this way the common cellular membrane beneath the pleura became so lacerable that the pleura itself was stripped off without much difficulty, and without inflicting any breach whatever in the aerial cellular structure of the lung, which it had covered. The lung thus divested of its pleura, presents to the eye, more or less distinctly, lines on its surface, which indicate the situation of what may be called the *pulmonary fissures*—a term more correctly applicable than that of interlobular, inasmuch as by the term interlobular is usually understood a something situated between either the longer lobes or smaller lobules; whereas by the term *pulmonary fissures* is meant certain spaces, occupied by common cellular membrane, and which descend from the surface towards the interior, but without penetrating the aerial cellular tissue of the lung; thereby dividing more or less deeply the surface of the organ into a number of insular portions, some of which may comprise a great number of lobules. Guided by the linear indications on the surface of the now naked lung, we can in general, with the aid of a pair of points let into handles, or a pair of fine scissors, and without much difficulty, succeed in laying open and exposing the pulmonary fissures, at the bottom of which, merely surrounded by a loose cellular membrane, and resting on the unbroken aerial pulmonary tissue, we discover a vessel; that vessel is the pulmonary vein, alone, and unaccompanied by any artery whatever. This vessel may be distinctly traced from larger to smaller trunks towards its source, until we reach the common cellular membrane between the ultimate lobules, from the exterior of which the vein appears to originate; whilst on the other hand, by continuing the mechanical operation towards the root of the lungs, we, with almost equal facility, trace the vessel, still lying at the bottom of the pulmonary fissures, and becoming gradually larger and larger by the addition of branches, which proceed into the pulmonary fissure, and are derived either from the neighbouring smaller pulmonary fissures, or from the uniting cellular membrane between the ultimate lobules themselves, until at length it joins the large trunks at the root of the lungs, to form the great pulmonary veins. A small artery is not unfrequently observed running across the pulmonary fissures, from a portion of lung on one side to a portion of lung on the other; and in one instance I have found an exceedingly narrow strip of healthy lung passing like a bridge across the fissure, on the very surface of the lung."—Pp. 151—153.

He then presents a short summary of the inferences, which he thinks are established, regarding the minute vessels of the lung in the following words:—

"Thus, then, the human lung may be said to be made up essentially of a vast expanse of membrane, the interior of which, during the whole of extra-uterine life, is unceasingly exposed to the influence of atmospheric air, and upon the surface or in the substance of which are spread out the capillary ramifications of the pulmonary artery; these arterial capillaries passing from thence to the exterior of the membrane, to form the pulmonary vein, which throughout its whole course is found to be situated on the exterior of the ærial cellular structure of the organs."—P. 153.

Mr William Addison gave in 1842 an account of the results of various researches which he had made by the aid of the microscope, on the distribution of the ends of the bronchial tubes, and arrived at the conclusion that these tubes do not end in closed sacs; and that, after dividing into numerous minute branches, which take their course in the cellular interstices of the lobules, they terminate in their interior in branched air passages, and freely communicating air-cells.

It is proper to premise, that to each lobule of the lung belongs one bronchial tube of some size; that the divisions or branches of this bronchial tube are confined to the lobule to which it belongs, which indeed it forms, and do not communicate with the branches of adjoining lobules; and that each lobule is enclosed in cellular or filamentous tissue, firmer than that within the lobule, and which thus separates that lobule from the surrounding ones. The small bronchial divisions within each lobule are *intralobular ramifications.*

In the fœtal lung, these intralobular bronchial divisions pursue a regular branched arrangement, subdividing in all directions, and terminating at the boundary of the lobule in closed extremities.

Many also terminate in the interior of the lobule. These intralobular branches do not anastomose.

In the fœtal lung, there are no air-cells properly speaking. But when an animal breathes, the air entering by its pressure into the windpipe and bronchial tubes, proceeds to the intralobular branches; and in this manner distends each lobule speedily to as great extent as these intralobular branches allow. After this they are found to form a series of communicating cells, which are permanently occupied by air; and in this all the trace of the original branched arrangement is lost or obscured. These cells are pentagonal or hexagonal in shape.

This may be regarded as the statement of the fact as given by Mr Addison. The cause of this cellular formation is twofold; first, the forcible pressure of the air which enters by the windpipe, and is perpetually impelled to the ultimate extremities of the tubes and their intralobular ramifications; and the delicate and yielding membrane of these ramifications, which, by presenting an unequal resistance, is thus distended into cells.

The air-cells do not, however, communicate with each other in the interior of a lobule in an indiscriminate and general manner. As the intralobular bronchial ramifications do not anastomose, the air-cells formed along one branch do not communicate with those formed along another; and so on through the whole lobule.

The lobules nevertheless present in their interior branched passages forming a communication between the cells. But these passages are stated to be neither tubular nor cylindrical. They are denominated lobular passages.

When a thin section of inflated and dried lung is placed under the microscope, a number of large well-defined oval FORAMINA, with a sharp delicate edge, is seen thickly distributed among the cells. These FORAMINA are portions of lobular passages. They are smaller near the pleura and surface of the lung than in the interior of the organ.

The facts now stated, Mr Addison represents to be established by the results of experiments which he made on the lungs of the ox. As these experiments do not admit of abridgement, we insert two of them in the words of the author, in order to obviate the chance of misrepresentation.

"*Experiment* 2.—I injected with mercury a small bronchial tube in the lung of an ox, leading towards the thin margin of the organ; the metal appeared at the surface, forming a mass of minute globules. Having made an incision in the interval between two lobules, and inflated the cellular tissue, I was enabled carefully to dissect away the pleura, and I then observed through a lens that the globules were contained in delicate membranous sacs, forming rounded eminences projecting from the tissue. I then separated several lobules from each other, and saw the mercury at the surface of every lobule, presenting rounded eminences similar to those observed at the surface of the lung. On examining these rounded eminences or globules in the microscope, I perceived that the mercury was not inclosed in a simple sac or cell, but in a divided or multilocular cavity. Nor is it difficult to comprehend the character of these multilocular cells at the surface, when we conceive the nature of the structure of a lobule, consisting of numberless small branches of a ramified tube, which the atmosphere at birth distends into cells. The extremities of these branches, evolving or shooting forth under the pressure of the air, meet with resistance and support from the adjoining lobules, and being, as it were, thrown back upon themselves, form the multilocular cavities or cells I have described (*b*, *c*′) fig. 8. WAGNER's figure represents these terminal cells only.*

"*Experiment* 3.—Having inflated a recent lung, I cut off a small portion, and examined it as an *opake object* by the microscope; I found all the oval foramina occupied by large air-bubbles, other bubbles of various sizes occupying the surrounding cells. I then placed *a very small* piece between two slips of glass, which were so arranged under the microscope, that I could gently and gradually press them together, still keeping the object (*now viewed transparent*) in focus. I then observed the air-bubbles changing their situation, not by moving equably through any tube or cylindrical passage, but by sudden starts from cell to cell. I frequently saw a large bubble of air become compressed for a moment in passing from one cell to another, and sometimes divide into two smaller bubbles, one of which passed on to another cell, the other retiring to the spot from which a momentary pressure had removed it. I have frequently watched a bubble of air pass through three or four cells in succession, the communication between them not being through a tubular passage, but by limited openings (oval foramina) leading from cell to cell.

"It does occasionally happen that a small portion of bronchial tube may be included in the object thus submitted to examination, and if so, when the glasses are pressed together, the air-bubbles glide easily and readily through it. The bubbles of air formed in the lungs are of all dimensions, some large, some small, and others so minute as not to measure more than $\frac{1}{1000}$th or $\frac{1}{800}$th of an inch in diameter; hence three, four, or more may occupy a single cell, and the heterogeneous adhesion between them and the tissue is so strong, that it is impossible to expel all of them by pressure. They may, however, be removed from very thin sections of recent lung by two or three days' maceration in water, and the pulmonary network is by this means rendered very distinct; and if such sections be carefully examined by a lens, *lobular passages may be seen partially laid open, disclosing a series of communicating cells* (*a*, *a*, *b*, fig. 3.)" Pp. 159, 160.

When mercury is poured into the lungs of a rabbit which have been macerated, so as to expel the air, it gets into the air-tubes, and appears at the surface of some in the form of globules, at that of others as beaded and nodulated branches, which, according to Mr Addison, combine the character of cells and passages.

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* Icones Physiologicæ, tab. iv. fig. 8, 1839.

The membrane of these air-cells, when examined by the microscope, does not form round or even rounded cells, but flat membranous plates, circumscribing polyhedral spaces. They present ovate bodies as part of their structure. They possess an epithelium in the form of large, round, nucleated scales, in each of which from one to fifteen or more nuclei may be counted.

Mr Addison found, like Magendie, the cells in early life small, and in old age large. At the age of 45, they vary from $\frac{1}{200}$ to $\frac{1}{600}$ part of one inch in diameter.*

The correctness of these statements has, on the whole, been confirmed by the researches of Mr George Rainey.

Mr Rainey finds, that when the bronchial tubes have arrived at about one-eighth of one inch from the surface of the lung, the membrane terminates abruptly; that the passages conveying the air, which he terms *intercellular*, continue in the same direction as the tubes of which they are continuations, but without perceptible membranous lining; that the diameter of the ultimate bronchial tubes is from $\frac{1}{50}$ to $\frac{1}{30}$ of one inch; that they communicate with but few cells; that the intercellular passages, the intralobular of Mr Addison, are at first of a circular form, communicating also with few cells; but as they approach the surface of a lobule, their number increases, and at length these communicating openings are so numerous and so close, that the intercellular passage loses its circular figure, and forms an irregular-shaped passage running between air-cells, and communicating with them in all directions, and having arrived at the surface of a lobule, it terminates in an air-cell, which is not dilated, but is in truth of the same size as the passage.

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* On the Ultimate Distribution of the Air-Passages and the Air-Cells of the Lungs. By William Addison, Esq., F. L. S., &c. Read 7th April 1842. Philosophical Transactions, London, 1842.

On the use of the term vesicle, Mr Addison pronounces a criticism which partakes more of boldness than wisdom or knowledge. "Anatomical writers," says Mr Addison, "generally use the terms air-vesicles and air-cells synonymously, so that they are convertible terms; but, strictly speaking, an air-vesicle is an air-bubble, and may exist either in or out of a pulmonary air-cell."—Phil. Trans. vol. for 1843, p. 158.

It is rather the term vesicles than air-vesicles, that anatomical writers use as synonymous with that of air-cells. By what means Mr Addison has arrived at the inference that vesicle means air-bubble, it is not easy to see. But, with deference to Mr Addison, he will find that all the anatomical writers who have spoken of these bodies—Malpighi, Winslow, Senac, Haller, and Soemmering—employ the term in its original acceptation, that is, a small *vesica*, or bladder, or small membranous bag or cell. The words of Soemmering are; "*Pulmonum substantia* parvis *cellulis*, *vesiculis*, vel *succulis* aere plenis conflatur."—Plures ejusmodi vesiculæ in acervulos congeruntur. Tomus sextus, § xxi.—The idea that the word *vesicula* is ever used to signify an air-bubble is a modern invention, not sanctioned by classical use. *Bulla* is the word used.

With regard to the existence of epithelium lining or forming these cells, different observers entertain different opinions. The views of Mr Rainey may be best stated in his own words:—

" The epithelium (ciliated) which is peculiar to the air-passage, lines the bronchial tubes as far as the termination of the bronchial membrane, but it does not appear to extend beyond the membrane into the bronchial intercellular passages, or into the air-cells. The smallest tube in which I have seen the ciliated epithelium, had a diameter of about one-thirtieth of an inch, but I have never found it in a tube where the bronchial membrane was not present also. As the lung near its surface is made up entirely of air-cells and intercellular passages, the contents of these can easily be examined, without there being any admixture of the epithelial lining of the bronchial tubes, care being taken only to select for examination those portions of the lung which lie immediately beneath the pleura. I have repeatedly examined those parts of the lung, but have not found this kind of epithelium. In the healthy lung, black pulmonary matter is present in greater or less abundance in the air-cells, as well as in the cellular tissue, distending the interlobular fissures, and adhering to the external surface of the blood-vessels; and, in the diseased lung, granular matter may sometimes be found in the intercellular passages and air-cells, but it has not the character of ciliated epithelium.

" In the uninjected lung, the remains of nucleated cells in the walls of the capillaries have a sufficient resemblance to epithelium to be easily mistaken for it. Mr Addison seems, from the following observation, to have fallen into this error:—' They (the air-cells) possess an epithelium, in the form of round nucleated scales, and from one to fifteen or more nuclei may be counted in a single scale.' Mr Addison, however, adds,—' But I have never satisfied myself that they possess the ciliated cylinder epithelium, so abundant in the trachea and bronchi.'*

" From what has been stated, it appears that the upper part only of the air-passages is lined by mucous membrane, and it will be shown hereafter that those parts in which the aeration of the blood more particularly takes place, are lined only by a very thin fibrous membrane. These facts seem to agree with the phenomena presented by acute inflammation of these structures, inflammation of the bronchial membrane (bronchitis) being attended by the symptoms peculiar to inflammations of other mucous membranes, and inflammation of the membrane lining the air-cells (pneumonia) being accompanied by deposition of fibrine, as in inflammation of the common fibro-cellular tissue.

" The existence of a difference in structure between the membrane lining the bronchi and that lining the air-cells was long ago inferred, and insisted upon, by Dr Addison and others, from pathological considerations; but, I believe, the fact has not been *demonstrated*."—Pp. 583, 584.

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* Philosophical Transactions, 1842, Part II. p. 162.

The air-cells are small, irregularly shaped, yet most frequently four-sided cavities, varying in size in different parts of the same lung. Those are smallest, as well as most vascular, which are situate nearest the centre; while their size increases, and their vascularity diminishes, as they extend into remote parts. The air-cells situate close to the bronchial tubes, or intercellular passages, open into them by large circular apertures; while those placed further from these passages, communicate with them through the medium of other cells.

Besides these intervening air-cells, there are others which fill the angle formed by the bifurcation of the intercellular passage, and which thus appear to form a communication between them.

> "It is very easy, in an injected preparation, to see the communication between two, or sometimes three cells; but to determine with exactness the number of cells which communicate in succession, is probably impossible, in consequence of the section which is favourable for displaying the opening of one, being unfavourable for displaying that of the other; but, that the communications between the cells of the same lobule are very free, is obvious from the fact, that, if injection be thrown into a small bronchial tube, it will distend all the cells of that lobule, while none of the injection will pass into the adjoining lobules. In the mammal, the walls or partitions by which the air-cells are separated from one another, consist of a single plexus of vessels enclosed in a fold of membrane, whilst the sacculi of the lung of the reptile, which may be considered to correspond to the air-cells in the lung of the mammal, are separated by a plexus folded on itself, and, therefore, consisting of two layers of vessels, a character by which the lung of the reptile differs from that of the mammal.
>
> "The plexuses in the lungs of the reptile and mammal consist alike of a very dense net-work of capillaries, into which terminal branches of the pulmonary arteries terminate, and from which the radicles of the pulmonary veins take their origin.
>
> "In the latter—the mammal—the number of capillary plexuses is not, as some have supposed, the same as that of the air-cells, that is to say, a terminal artery does not divide into a plexus at any particular part of a cell, its branches uniting for the commencement of a vein on the opposite part. On the contrary, one plexus passes between, and supplies several cells. In the interior of the lung, the exact extent of an individual plexus cannot be determined, in consequence of the removal of some part of it by the section necessary for its exhibition: But, on the surface of the lung where the extent of these plexuses, in relation to the cells over which they

ramify, can be easily made out, an individual plexus may be seen to spread over an area of ten or twelve cells in some parts, and fewer in others, the exact number depending, in some measure, upon the size of the cells.

Here, after the pleura has been removed, the terminal arteries may be observed descending from the interior of the lung between the cells to the surface, where they send off their branches in all directions, some anastomosing with the adjacent branches of the pulmonary artery, and others terminating in the radicles of the pulmonary veins.

"On that side of a cell in which is situated the opening of communication with an adjoining one, the capillaries anastomose all around this opening, so as to appear in the injected preparation, by reflected light, to form its immediate boundary; but by transmitted light this opening will be seen to be formed by circular threads of the lining membrane of the air-cells, which extend beyond the circle of vessels. To these communications, Mr Addison has given the name of "lobular passages," under the idea that no air-cells exist in the fœtal lung, but that they are formed after birth by the mechanical dilatation of the bronchi.

"The incorrectness of this notion is obvious from this fact, that the air-cells, with the capillary plexuses between them, are perfectly developed in the lungs of animals which have died two or three days after birth; for, in such cases, it cannot be supposed that these vessels could have been formed in so short a time as that which had elapsed between the birth and death of the animal, even had the parietes of the bronchi been dilated in the manner supposed by Mr Addison."—Pp. 586—588.

Mr Rainey, accordingly, controverts the statement made by Mr Addison, that, in the lungs of the fœtus, the air-cells are not developed. He injected the lungs of various fœtal animals which had never breathed, and found, on examining them with the microscope, that the air-cells were developed proportionally with other parts of the lungs.

He adds, however, that, in the very young fœtus, the *septa*, or partitions between the air-cells, consist almost entirely of minute cellules or granules, and a small quantity of fibrous tissue, with scarcely any blood-vessels; and that, as the age of the fœtus advances, this granular matter diminishes, while the capillaries increase, so that at birth, the same arrangement of the air-cells and the other parts of the lungs is observed as in after life.

The capillaries of the lungs are situate, or contained within, a fold of membrane. Traced from the peripheral to the central parts, this membrane lines first the air-cells, which are next the surface of a lobule, whether next the pleura or adjoining lobules; then the cells enclosing the capillary vessels; and thence extending from cell to cell, it arrives at the intercellular passages, and at the termination of the bronchial tubes becomes identified with the bronchial membrane.*

Subsequently to the date of the researches now mentioned, M. Alquié of Paris has made, by means of metallic injections, sundry observations on the terminal divisions of the bronchial tubes, which also deserve to be made known. His object was to ascertain by these means the shape of the spaces or cavities, into which the air is insinuated during respiration. Do the air tubes terminate in shut sacs, or are they expanded into *ampullulæ*, or do they give rise to tortuous labyrinthine canals, as Bourgery represents? If the bronchial canals terminate in vesiculæ, do these communicate with each other laterally, and in any other mode than by their point of origin? In order to determine these points, M. Alquié poured into the trachea of a sound lung a fusible alloy, consisting of eight parts of bismuth, five of lead, three of tin, and half a part of mercury.

The liquid alloy enters first only the first divisions. But by immersing the lung in warm water, which is soon carried to the boiling temperature, and allowing it to remain about ten minutes, the alloy is kept fluid, and gradually descends and expels the air, it is not known how. At length the alloy penetrates to a sufficent depth, and it is supposed then that it reaches the extremities of the bronchial tubes, particularly when the lung being removed from the basin, and placed on a horizontal support, its contractility assists, as the temperature falls. The next object is to detach from the pulmonary texture this impression of the respiratory canals, and this is done in the following manner.

The injected lung is immersed in a strong solution of caustic potass, which, though not affecting the metal, dissolves the parenchyma so completely, that at the end of two or three days it entirely disappears. The rest of the pulmonary tissues are then washed away by the aid of a stream of water, when a mould of the bronchial terminations is left.

This operation furnishes a correct metallic impression of the air-cell ramification. When its divisions are analysed, the observer beholds the rounded ends expanded into granulations. These expansions are sometimes isolated at the extremity of a twig; but most commonly they are clustered together to the extent of two, three, five, or more. The form of these aerial extremities is generally oval. When they are examined by a lens,† the surface presents irregularities which become more distinctly visible when examined by the microscope.

From this it appears that the bronchial extremities do not terminate in cylindrical closed sacs, but in vesicular expansions, and that there are no interlacing labyrinthine canals. It may also be inferred that the respiratory extremities are distinct and isolated; and that the vesiculæ or cells communicate with each other, only when they are fixed to the same bronchial twig, and have the same point of origin.

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* On the Minute Structure of the Lungs, and on the Formation of Pulmonary Tubercle. By George Rainey, Esq., M. R. C. S. Medico-Chirurgical Transactions, vol. xxviii. p. 581. London, 1845.

† Academie des Sciences 15 and 22 Novembre 1847.

In several points these statements agree; in others they greatly differ. On two points all agree. The first is, that within each lobule there is a separate or proper system of minute bronchial ramifications with terminal ends, between which there are communications within the lobule only. The second is, that these bronchial divisions and terminations, whether named air-cells or not, do not communicate with those of the contiguous lobules. The air which enters the interior of one lobule never can find its way into the interior of the contiguous lobules.

On some other points several observers only, not all, seem to agree. Thus the perforations and apertures in the ultimate bronchial divisions, mentioned by Reynaud, the sieve-like orifices seen in the walls of the tubes by Bourgery, and the foramina described by Mr William Addison, seem all to refer to the same objects. It seems also established, that though the bronchial tubes at their ends do not actually form dilated cavities or ampullulæ, as was maintained by Malpighi; yet, from the circumstance of several small bronchial tubes or cavities communicating with each within a lobule, this arrangement assumes the appearance of ampullulæ or dilated cavities. These facts seem to result from the observations of Dr Thomas Addison, M. Bourgery, Mr William Addison, and Mr Rainey, and perhaps from those of M. Alquié.

Lastly, it results from the observations of Magendie, Dr Thomas Addison, and perhaps we may add Mr Rainey, that it is doubtful if the existence of a membrane continued from the small bronchial tubes into the air-cells and vesicles can be demonstrated; and that the most minute divisions of the pulmonary artery and veins, or the pulmonary capillary system, are so ramified through this portion of the lungs, that these vessels and their tunics form, as it were, the communicating air-cells.

The great object of all correct anatomical knowledge is to enable us to understand physiological phenomena, and pathological processes. On the former it is unnecessary to say much or almost anything. The disposition already described explains most of the merely mechanical phenomena of respiration. It shows how the air which enters the windpipe must advance, by its elasticity and pressure, through the bronchial tubes to their extremities, and thus penetrate into the air-cells, which are never afterwards during life emptied of air. It is indeed, by the air remaining thus in these cells, that the changes are effected in the blood circulating in the pulmonic capillary system.

As to morbid changes, it must be allowed, as Dr Addison has observed, that while the anatomical structure serves to elucidate the nature of several of these changes, they in their turn may be employed to elucidate and explain the structure of the lungs. This is perhaps best seen in such diseases as lobular pneumonia, and tubercular deposition; and to these chiefly we confine attention at present.

Lobular pneumonia is a peculiar form of inflammation affecting the separate lobules of the lung, and often confined very strictly to one or more lobules, always limited very exactly to the lobular boundaries. As these lobules are bounded and invested by a proper filamentous tissue, so that the tubes and cells within one lobule do not communicate with those of the adjoining lobules, so the morbid action is confined at first strictly to one lobule, sometimes to a portion of one lobule; and if other lobules are affected, it is distinctly and separately from these.

The inflammatory or congestive action affects two tissues, the membrane of the cells, and the lobular filamentous tissue. In the first it causes exudation of plastic lymph, which fills the cells very much, in the same manner as takes place in the deposit of tuberculous matter. Indeed, they seem often the same. In the second it produces effusion of blood, bloody serum, lymph, and sometimes sero-purulent matter, so that the whole lobule seems like a whitish or grayish-coloured firm compact mass. These lobules, then, are felt like hard tubercular knots in the lung, the residue of which is soft, elastic, and crepitant.

Other changes follow. The pleura becomes inflamed over these lobules; and lymph in patches is deposited. The lobules themselves, or portions of them, may undergo a secondary ulcerative or destructive action, and the result is either pleuritic inflammation and adhesion, or perforation of the lung.

These changes proceed for weeks or months, until the whole of both lungs present a multitude of roundish or irregularly formed nodules about the size of small nuts, diffused through their substance. When these are divided by the knife, they present an exterior of reddish, firm vascular substance, inclosing in general small grains of grayish-coloured matter, sometimes like coagulated lymph, sometimes like purulent matter. They are manifestly confined to the minute divisions or lobules of the lungs. The lungs at the same time are infiltrated with serum; the bronchial tubes contain puriform mucus; the *pleura* is invariably more or less inflamed and covered with patches of albuminous exudation, especially opposite to those inflamed lobules which approach nearest to the *pleura pulmonalis*; the pulmonic and costal *pleuræ* are often united by soft recent adhesions; the upper part of the apex of the lung generally adheres extensively; and sero-albuminous or puriform fluid is found in the cavity of the *pleura*.

We shall not, however, dwell on this change, to which we directed attention formerly, when noticing the observations of Dr Addison on various forms of pneumonia.

We have seen that this author regards pneumonia as consisting in inflammation of the air-cells. It includes this as well as inflammation of the investing tissue.

Tubercular deposition is another morbid change, the nature of which can only be understood by bearing in mind the cellular or vesicular arrangement within the lobules.

The first person who appears to have looked to the air-cells as the parts of the lungs originally affected by tubercular deposition, was Magendie, who made in 1821 some interesting observations on this process.

According to Magendie, the first indications of tubercular phthisis consist in the deposit of a certain quantity of grayish-yellow matter, in one or more cells of the lung. The yellow matter sometimes fills completely and distends the cells; but it is easy to perceive the small blood-vessels, which circumscribe the matter deposited. In other instances the yellow matter is moveable within the cells, and may probably be capable of being expelled from them.

In some instances only one or two cells contain yellow matter; but most frequently this fills all the cells forming one lobule. In this case the matter adheres to the small vessels; and these soon disappear; on which the whole lobule seems formed by yellow or tubercular matter.

After opening numerous bodies, M. Magendie never saw in the cells the small pearly granules which, according to certain authors, are the first germs of phthisis. On the contrary, the matter first seen is that named tubercular matter, which is presented as if secreted by the walls of the small pulmonary blood-vessels.*

These views on the first origin of tubercular deposition were afterwards elaborated and illustrated by M. Schroeder and Dr Carsewell: and they seem from various facts to be most probable.

In a former volume of this Journal,† a very detailed account of the researches of Schroeder was given; and, while we refer to that for specific information, we may add that since that time observations have been made on the same subject by M. Ravin, Dr Nugent Kingston, M. Lebert, and Mr Rainey. The observations of Dr Kingston we partly considered when they appeared.* To those of M. Lebert we may afterwards direct attention. At present we subjoin from the paper now under consideration, the account which Mr Rainey gives of this morbid state, its origin and progress.

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* Memoire sur la structure du poumon de l'homme, &c. &c., et sur la premiere origine de la phthisie pulmonaire; par M. Magendie. Journal de Physiologie, Tome I. p. 78. Paris, 1821.

† Vol. xlv. p. 500. 1836.

"In order to determine with accuracy the precise situation of tuberculous matter, and to observe the manner in which the lungs become progressively destroyed by its presence, it is advisable to examine tuberculous lungs which have been successfully injected.

"In such preparations, when viewed by reflected light, the pale colour of tuberculous matter contrasts so strikingly with the red colour of the injection in the capillaries of the walls of the cells, that there is no difficulty in discerning its exact form and limit, even in quantities so small as to fill only one, or even a small portion of a cell.

"By this mode of examination, it will be clearly seen that the tuberculous matter is poured from the free surface of the pulmonary membrane into the interior of the air-cells. These becoming distended, and the septa between the contiguous cells being at first compressed, and their vessels afterwards obliterated, the supply of blood to the diseased part is cut off, and a tubercle formed, corresponding in size to the number of distended cells.

"The manner in which tubercles extend themselves to the parts adjacent is best seen by observing the progress which had been made by the deposition in the air-cells situated in their vicinity. Here some of these cells will be seen to contain only a small quantity of deposit, others to be completely distended with it, though their vascular walls remain entire. Between other cells containing tuberculous matter, these vessels will be seen to have become partially obliterated; and lastly, in the septa, between those cells where the accumulation has amounted to such a quantity as, generally, to compress the capillaries, and probably cause their absorption, no vessels are left, and nothing exists but the pulmonary membrane, which remains mixed with the tubercular matter, and which (the tubercle having been broken up) is in a state to be ejected from the lungs in the expectoration, in which it can be detected by the microscope.

"An imperfect examination of the vascular connexion of a tubercle with the surrounding air-cells, in the injected lung, might give the idea of vessels passing into it from the adjacent unaffected parts, and thus lead to the supposition that it is vascular,—an idea which has long been prevalent among pathologists. A careful examination, however, clearly shows that these vessels are merely portions of plexuses which have not yet become absorbed. In most cases, these vessels will appear as arcs of circles of greater or less extent, of the same radius as the plexuses between the air-cells, of which they are, obviously, the remains.

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* Vol. xlix.

"The deposit of tuberculous matter takes place in the bronchial intercellular passages at the same time and in the same way as in the air-cells; and their walls—which are in reality the air-cells between which they pass—disappear in the same manner as those which separate one cell from another; and thus these passages being occupied by tuberculous matter, contribute to form a part of the tubercle. The smaller bronchial tubes also becoming distended with tuberculous matter are involved in the general mass.

"It will be obvious from what has preceded, that as a tubercle increases in size, the central parts of it will become further and further removed from those vessels, by which the tuberculous matter was in the first instance deposited, and afterwards maintained in a state of vitality, and consequently these parts will have the greatest tendency to lose their cell or vegetable life, and become softened. According to this explanation, this process ought always to begin in the centre of a tubercle, and I believe this is generally the case. But it must be recollected that the geometrical centre of the mass is not necessarily the point furthest removed from the source of circulation; the point may even be on the side of the tubercle, provided it be the remote cells of a lobule which are occupied by the tuberculous matter, namely, those bounded by an interlobular fissure: in such a case, this part of the tubercle might be further removed from the source of circulation, than its exact centre.

"Some pathologists contend that tubercle is the result of inflammation. Without entering at length upon the consideration of this question, which is one *de verbo* and not *de re*, I may state that the perfectly natural appearance of the vessels close to a tubercle, and even of the cells containing a small quantity of tubercular matter, not sufficient to have impeded their circulation in the capillaries during life, when compared with the tortuous and unequally dilated state of vessels going to air-cells filled with fibrine, in consequence of inflammation, are pathological considerations in favour of the non-inflammatory nature of phthisis. These facts go also to show that the obliteration of the capillaries between the air-cells is, in phthisis, produced by some force which has exerted upon them a slow and very gradual compression, such as can only be conceived to have been produced by the accumulation of tuberculous matter, in contiguous cells, pressing upon the intervening plexuses.

"I may observe, that the facts I have mentioned refer only to the most ordinary description of phthisis, and that they are the result of a great number of examinations; and I contend that they prove that this form is not at all connected with other forms, and that it is erroneous to suppose that miliary is necessarily the incipient state of common tubercle. My preparations, as well as those of Mr Quekett, show that the progress of this tubercle, from its commencement up to its perfect formation, depends simply upon the quantity of the deposit; for it can be seen, in some parts, occupying only a part of a cell—in others, one, two, three, or even of an indefinite number, and, in every case, to exhibit the same microscopic characters.

"In this form of phthisis there appear to be two pathological states from which we can deduce an explanation of its symptoms:—first, a portion of lung is rendered impermeable to the blood; secondly, the blood is thrown upon the surrounding unaffected parts.

"Whether the first of these states ought to be regarded as inflammation or not, must ultimately remain a matter of opinion, so long as the term '*inflammation*' has no definite signification.

"Without doubt, it is to the second state as a mere consequence of the first, that the attacks of hæmoptysis are due, as well as the susceptibility to severe and obstinate catarrhal symptoms. The tuberculous matter, acting like an extraneous body, will produce a constant tendency to inflammation, and, after it has been produced, prevent its removal. Hence will follow those hectic and other symptoms which result from the continued operation of any irritating body.

"As regards the expectoration, this will occur most frequently from the bronchial membrane, and, most probably, is not to be distinguished from that in ordinary bronchitis. It will be only during the breaking up of a tubercle that matter truly tuberculous will be expectorated, and this, I believe, can be recognized with certainty by no other character than its containing fragments of the membrane of the air-cells."—Pp. 590—595.

From the foregoing account it results, that the observations of Mr Rainey on the deposition of tubercular matter correspond with and corroborate those of Magendie and Schroeder.

Various other disorders might yet be advantageously considered as explicable, if not entirely, at least in part, by the cellular structure of the lobular bronchial tubes. *Bronchitis*, for example, is a disease, the fatality and peculiar effects of which, especially those of the vesicular variety, cannot be understood without correct ideas on the arrangement of these cavities. The same may be said of one of the effects of *bronchitis*, namely, enlargement and rupture of the cells, and emphysema of the lungs, which is a lesion entirely confined to the air-cells, and their surrounding filamentous tissue. The effects of the inhalation of smoke, coal-dust, and minute particles of all kinds, are equally to be considered as imperfectly intelligible without this knowledge.

We shall not however extend this article further, and we merely give those general inferences on pathological states of the lungs which Dr Thomas Addison makes in the paper, the title of which is placed at the beginning of this article.

"6thly, That pneumonia and inflammatory tubercle are identical; 7thly, that acute pneumonia in moderately good constitutions scarcely ever leads to the formation of an abscess, unless deposit previously existed; but that when it occurs in cachectic or broken down constitutions, or supervenes in the process of chronic or organic diseases, it occasionally causes one or more distinct and separate lobules to soften down into an ill-conditioned abscess; 8thly, that ordinary tubercles present the same varieties in the lungs as they do in serous membranes; 9thly, that emphysema of the lungs consists chiefly of mere dilatation of the cells, but in part also sometimes of more or less extensive laceration of them; and lastly, that the circumscribed gangrene of Laennec is commonly, if not uniformly, a mere effect or advanced stage of pulmonary apoplexy."—Pp. 148, 149.

Art. V.—*Births and Deaths Registered in London during the Weeks ending Saturday,* 13*th November,* 20*th November,* 27*th November,* 4*th December, and* 11*th December* 1847, (No. 46, 47, 48, 49 and 50). *Annual Series* viii. Published by authority of the Registrar General.

We have had, in the course of sixteen years, three epidemic visitations of influenza. One was in the spring and beginning of summer 1831; the second was in the winter of 1837; and the third is at present displaying its comprehensive prevalence in Paris, London, Edinburgh, and other large towns.

In this city its attacks have been most general, causing schools and other places of education to be temporarily shut; and producing among the aged and those affected with chronic pulmonary disorders, considerable mortality.

In London it has been both very general and caused great mortality, as will appear from the following statements derived from the Reports of the Registrar General.

Amidst a population bordering on two millions of souls, the total mortality of London for each of the five weeks ending at the dates given below, amounts, under the following heads, to the number specified:—

	Total.	Influenza.		Pneumonia.		Bronchitis.		Asthma.		Phthisis.	
13th Nov.	1098	4	3	79	109	58	39	18	24	121	134
20th Nov.	1086	4		95		61		9		108	
27th Nov.	1677	37		170		176		77		153	
4th Dec.	2454	198		306		343		86		198	
11th Dec.	2416	374		294		299		78		192	

These numbers show a very large augmentation in mortality during the three weeks, from the 20th November to the 11th of December. The numbers at the top on the right show the average number by the five diseases, influenza, or catarrh, bronchitis, pneumonia, asthma, and phthisis. All these diseases are allied; and during an epidemic influenza, persons affected with asthma are very liable to have all their symptoms aggravated to a fatal degree, while numbers of individuals who labour under chronic *bronchitis*, or catarrh, winter cough, emphysema of the lungs, or in short, any chronic disease in these organs, are liable, under the epidemic influence, to have either an acute attack, or all their usual symptoms converted into diseases which terminate fatally.

It appears from the numbers above given, that, previous to the 20th November, no excessive mortality was observed. Indeed at this time deaths from pneumonia and asthma are below the average. Immediately after the 20th November, the mortality undergoes a sudden and considerable increase: twelve times the average in influenza, nearly double in pneumonia, five times in bronchitis, and three times in asthma. It is to be observed, however, that during the two weeks ending 13th and 20th of November, the mortality from *bronchitis* is very much increased.

From this it is to be inferred, that the disease had already commenced during the second two weeks of November, and was showing its effects by the increase in the number of cases of bronchitis. It further appears, that at this time the mortality was chiefly in children beneath 15, and aged persons above 60. Thus, of the 58 dead during the week ending 13th November, 29 cases are under 15 years, and 21 cases above 60 years; and in the week ending 20th November, of the 61 fatal cases, 31 are under 15 years, and 19 above 60 years. The morbid influence was therefore in all probability already in operation.

During the last week of November it was evidently acting with greatly increased force; and it is worthy of remark, that the augmentation of intensity is evinced not so much in the cases of influenza, as in those of pneumonia, bronchitis, and asthma, all of which as already stated, are very much increased. It is further to be observed, that with this increase in the mortality by pulmonary diseases, the general mortality is increased by more than one-half above that of the three first weeks of November.

In the beginning of December, the morbific force is still more intense on all the heads; and at this time, influenza, properly so called, has a large share in the general mortality. It is 66 times the annual average. The other three are likewise increased, though in unequal degrees. Pneumonia is three times; bronchitis eight times; and asthma is rather more than eight times above the average. The general mortality is also higher than that of the previous weeks by much more than double.

It appears, further, that during the whole of these weeks, the weekly average is higher than the weekly average for several preceding years. That average is 1046; and in the third week of December it starts at once to 1677, and in the first two weeks of December is above 2400 persons.

During the second week of December, which is the last of which we have an account, the epidemic continues much as in the previous week, changing only in a slight degree, the mode in which it operates. The general mortality is in a slight degree reduced; 38 in 2450 persons. This reduction appears most in bronchitis; in a slightly smaller degree in pneumonia and asthma, and not at all in influenza proper, which is now increased to what may probably be its maximum, being 374 persons in the week, or between one-sixth and one-seventh of the total mortality.

Such are the results to the end of the second week of December, namely, the 11th, beyond which the tables are not yet published. It would be rash to draw any conclusions regarding the subsequent career and effects of the epidemic. But as influenza rarely extends in such force as that now noticed, beyond four weeks, it may be presumed, that it has now attained its height; and that from this point of culmination, it will gradually or rapidly decline. Influenza, however, is a disease which always leaves after it much mischievous effects on the lungs in the shape of chronic *bronchitis*, asthma, emphysema, and sometimes phthisis; and the final effects of the present epidemic may not entirely cease for weeks or months with some persons.

Upon another disease of the lungs, influenza usually exerts a great and most hurtful influence. This is phthisis. The number of deaths from this disease, with the annual average of five years on the right, are given next to asthma; and from these numbers it appears that the mortality has been augmented during the three weeks of the greatest intensity of the epidemic. The increase during the last two weeks is about 50 per cent.

Measles and hooping-cough appear in like manner to have undergone a considerable increase in mortality during the same time. But this is not so remarkable as in other diseases.

The mortality of typhus has been increased during the first and second weeks of December to nearly four times more than the weekly average of five years. That average amounts to 36 deaths. But in the week ending 27th November, the number of deaths was 87; in that ending 4th December, it was 132; and in that ending 11th December, it amounted to 136. The majority of these deaths are among persons below the age of 60; nearly one-half in the second week, 61,—being between 15 and 60, and the same number, 61, being in the third week under 15 years.

The reason of this mortality during influenza is obvious. Typhus in this country, and at this season, has a very decided action on the bronchial membrane, and is liable to prove fatal by *bronchitis*. So much more likely is it to do so under the operation of an epidemic such as influenza, which affects severely that membrane and the circulation and secretions of the lungs in general.

In order to communicate a just idea of the total increase of mortality for the three weeks between 20th November and 11th December, we give the following summary of the deaths from influenza, pneumonia, bronchitis, asthma, and phthisis, during that period, compared with the average weekly mortality from all these diseases for five years. The average weekly mortality from the diseases now mentioned amounts to 309.[*] For the first week of the epidemic it is 632; for the second week it rises at once to 1131, nearly one-half of the whole weekly mortality, and about 100 more than the total weekly mortality of ordinary times. In the third week it is 1237,—more than one-half of the whole mortality of the week.

During the last two weeks specified, 4th and 11th December, it is further to be observed that the mortality from influenza affected in the greatest degree persons above 60. The mortality during the first of these weeks being four-sevenths of the whole, and, during the second, nearly one-half.

There are no means which enable us to speak with precision as to the districts of the metropolis, in which the epidemic has prevailed with greatest severity. The virulence also and mortality must depend partly on the density and kind of the population, and in a still greater degree on the previous average health of the inhabitants. The only circumstance of this kind which the tables furnish, is contained in the report for the second week of December, ending on the 11th; and from that it appears that considerable numbers of aged persons died of the disease in St Marylebone, in Whitechapel, and at Greenwich.

On the causes of this epidemic we find no information of moment. The temperature and the state of the atmosphere, as indicated by the barometer previous to the outbreaking of the epidemic, were much as usual. The only meteorological fact of much moment is noticed to have taken place about the end of the first and beginning of the second week of December (7th), when the barometer was only 28.381 inches; a depression which was doubtless connected with the violent gales which then took place.

ART. VI.—*Clinical Facts and Reflections; also, Remarks on the Impunity of Murder in some cases of Presumed Insanity.* By THOMAS MAYO, M. D., F. R. S., Fellow of the Royal College of Physicians; late Fellow of Oriel College, Oxford;

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[*]	1.	2.	3.	
Influenza, -	36	198	374	3
Pneumonia, -	170	306	294	109
Bronchitis, -	196	343	299	39
Asthma, -	77	86	78	24
Phthisis, -	153	198	192	134
	632	1131	1237	309

Physician to the Infirmary of St Marylebone. London, 1847. 8vo, pp. 217.

A considerable portion of the present volume has been already before the profession in the form of detached contributions in the London Medical Gazette. This, to a certain extent, renders it less necessary to notice its contents in detail; and even might justify us in passing it over altogether. There are, however, various subjects brought under consideration, to which, for several reasons, it may be proper to give some short and passing attention.

Dr Mayo, impressed with the value of the experience of different physicians in ordinary, not extraordinary cases, thinks it important to the progressive improvement of the healing art, that the results of this experience be recorded. Dr Mayo reminds the reader that we abound in valuable monographs; and he is desirous, he states, to add to their number, whatever may be the quality of his contributions.

This remark is unfortunate and not exactly that which we should have expected from one who writes like a scholar and a gentleman. The truth is, that we do not abound in monographs, by which are meant treatises or essays devoted to the full consideration of one single subject in all its branches. Our literature is very poor in these productions, much more so than that of the French, who again have fewer monographs than the German writers, who are certainly the most prolific in this form of medical production. Neither does the present treatise come under the head of the class of monographs. It belongs, on the other hand, to that of general practical treatises on different subjects in pathology and therapeutics; and, were we permitted to coin a term expressive of this order of productions, we should call them polygraphs,—a name which it is unnecessary to explain to Dr Mayo. These treatises form the strength and the weakness of English medical literature. They are excellent as records of the observation and experience, as well as the reflections of their authors; and they form in many instances valuable contributions, illustrating and often modifying and rectifying particular principles.

The publication of Dr Mayo is not less varied and discursive than those of any of his predecessors and contemporaries. The author has observed carefully; he thinks with originality, and in general judiciously; and he writes in an agreeable and impressive manner.

The subject first considered is that of cases of *peritonitis.* On these we have only to observe, that the first was a case of insidious *peritonitis,* probably strumous, in a girl of 8; and that the third was *peritonitis*, either complicating or caused by *cirrhosis* of the liver. The patient had fluid within the peritoneum; and the abdomen was tapped, giving issue to twenty pints of fluid. This man died the first day after the operation. Ascites is not common after acute *peritonitis*, but a very common symptom and effect of chronic *peritonitis*, both ordinary and tubercular. In the present case it seems to have been the effects of the cirrhotic liver, which has an uniform tendency to produce serous effusion within the peritoneum.

In the second chapter Dr Mayo gives two cases illustrating the pathological characters of a particular form of purpura. The petechiæ in these cases took place on the mucous surfaces, as they are usually observed to do, and appeared to be connected with a morbid condition of the blood.

Next comes *gastrodynia*, in which he adverts to the unsteady and rather empirical character of the treatment pursued; and commends the use of opiates and sulphate of iron.

Ileus, which is the subject of chapter fourth, is, we submit to Dr Mayo, though good as a symptomatological name, improper as a pathological one. It comprehends, or rather conceals, many different states of the intestinal canal. In the first case given, the obstruction, for obstruction was the cause, consisted in a constricted state of the rectum, of which about ten inches were so narrow, as scarcely to admit a quill. The structure of the contracted portion or its consistence is not mentioned. In the second case the cause of constriction was different, though not an unusual cause of the symptoms of ileus. It was the presence in some part of the intestinal canal, (could it be in the biliary ducts) of an oval biliary concretion, about two inches long, on the expulsion of which, after symptoms of ileus for several days with great suffering, all the symptoms permanently disappeared.

It has often occurred to us, after having seen one or two instances of this form of ileus, to consider whether the gall-stone does not at the time of obstruction stop in the gall-ducts, causing there inflammation and sometimes ulceration; and in this manner passing into the intestinal canal. Dr Tyson records a case in which the patient died during the process; and Dr Scott of Cupar published in 1825, in the 23d volume of this Journal, a similar case, in which the patient died during the process of inflammation of the gall-bladder, with all the symptoms of obstruction of the bowels.

In the chapter on *kyanosis* is given the case of a female, aged 57, in whom this symptom was found to depend rather on absence of a septum entirely, than on the presence of an opening in the septum; the aperture of communication extending one inch and a quarter from below backwards, and half an inch in the opposite direction. The right ventricle was extraordinarily large, and its walls from three to four-eighths of one inch thick. The semilunar valves of the pulmonary artery are said to have been perfect. The walls of the artery, as well as those of the pulmonary veins, thickened, and the cavity distended. We should have expected the orifice or trunk of the pulmonary artery to have been contracted.

Of the second case related in this chapter we have only to say that we observe in the account of the morbid appearances, a clerical error in the statement, that the right ventricle was hypertrophied. It should have been the left ventricle hypertrophied. The heart adhered to the pericardium; and weighed 22 ounces.

Of the next two chapters on rose and intermittent disease we say nothing.

In the eighth chapter, the author treats of the connection between cerebral and pulmonary disease. In one case apoplexy, in another softening of the brain with bloody effusion, was associated with hepatization of the right lung. In one other case of disease of the brain, the lungs were in a state of red and gray hepatization; and in a fourth case, miliary tubercles were found.

The tenth chapter is devoted to hysteria and the hysterical diathesis. The eleventh contains observations on blood-letting in cases of congestion.

The subjects of the next chapters are *scarlatina maligna*, the treatment of scarlet fever, chronic cerebral and spinal disease, *meningitis* showing only vascularity, albuminous urine, *haematuria*, probable cæcal abscess, double consciousness, and puerperal insanity.

The most interesting chapters in the volume are the twentieth, twenty-first, and twenty-third, on insanity and its connection with crime, on the plea of insanity to excuse certain atrocious and criminal acts, and on the impunity of murder in some cases of presumed insanity. In the course of these chapters, Dr Mayo examines in different modes the cases of Oxford, Macnaughten, Dalmas, Captain Johnson, the Honourable Mr Touchet, and the young woman Martha Brixey, who cut the throat of the infant at Greenwich. On these subjects his remarks are ingenious, and will be read with interest. But we doubt whether it is possible to subscribe to their justice.

The subject is one of great difficulty, and has been discussed and examined in every possible light by so many persons of different degrees of authority, that we doubt whether it is possible to settle it in any general terms. After the observations by Pinel, Georget, and Marc especially, we think little remains to be said. Georget has discussed this question so amply and with so much judgment in various articles in the *Annales D'Hygiène*, and especially in his review of the noted case of Henriette Cornier, that we think it impossible at present to add a new fact or a new argument to the point. The present author is decidedly of opinion that the Greenwich murderess, as he calls her, is not at present insane. This we believe; and we have seen and known similar cases in which a person, however insane at one time, may not be insane at another. But he goes still further, and appears, if we do not misunderstand him, to think that she never was insane, and that she was instigated to the bloody and cruel act, which she perpetrated, by ill temper and splenetic waywardness. All this is very probable. But we doubt whether it would be for the credit of public justice, that a wretched young woman, liable to fits of ungovernable temper, should be publicly put to death, because she now shows no indication of deranged mind.

The truth is, that all crime, perhaps all deviations from correct and upright moral conduct, are indications of derangement; and it is often difficult to draw the line of distinction between madness and moral guilt. We admit also, as Dr Mayo maintains, that in hysterical and nervous females, who often show these outbreaks of ill-temper and ferocity, there is, when they chuse to exert it, considerable self-control, so that to serve a purpose they seem to be capable of restraining the animal propensities to mischief and destruction. The great evil is, we believe, to be found in the first place, in allowing so many persons of uncertain temper and excitable or vindictive feeling to be at large; and, in the second place, in the neglect of teaching them at an early period of life the duty of controlling their passions. As to the first point, it is certain that there are moving about in society, at all times, many persons whose quickness to take offence, whose pride, hasty temper, and occasionally vindictive feeling to avenge supposed injury or affront, render them so unfit to pass through life, without being more or less frequently on the verge of insanity, or for a shorter or longer time under the dominion of passion alone, that it would be much better for these persons themselves, and probably better for the world at large, were they placed in situations, in which they would not be exposed to temptation. As to the second point, all experience concurs to show, that among the unfortunate persons who have committed crimes so atrocious as to be tried for their lives, either in this country or in France, every one has been noted from early childhood for violence of temper, outrageous pride, incapability of opposition or contradiction, however moderate, and all had shown long before the final act which brought them before the public tribunal, gross selfishness and total disregard to the feelings of others, as well as complete want of principle in the means of gratifying either their own whims, caprices, or desires of any kind. The last act was only one of a long series of proceedings, the effect at once of the leading indulgence of passion, and ignorance, or disregard of moral principle.

Dr Mayo admits that capital punishment in the case of Martha Brixey would have been inappropriate; but he seems to think that she has got off too easily. He thinks that the total escape of Oxford has been most mischievous; and he condemns the jury who acquitted Dalmas and Captain Johnston. He seems further to think that a little hanging in the case of Macnaughten might have been a salutary lesson to various persons.

The orthodoxy of all this we very much doubt. Macnaughten was a confirmed madman; and all that was wrong was, that he was not confined many months before he shot Mr Drummond. How is it possible to believe in the sanity of a man, who thinks that a whole political party are in league to oppress him, and who imagined that the British ministry had nothing to do than combine to persecute a hard-working, but wrong-headed mechanic. Oxford was a poor contemptible idiot; one of those miserable beings who, from ignorance and folly, are incessantly doing foolish things. Of the case of Martha Brixey, we have already spoken. But we add, that we think it very doubtful whether the verdict in the case of the woman Ann Hunt is consistent either with law or medical evidence.

The truth is, that the varieties of what is called moral insanity, are so great, that each case almost requires a separate examination. One man slays another from a blind headlong love of killing and destruction; such, apparently, was Captain Johnston. Another attempts the same crime, because he thinks, like Mr Ovenston, that he is oppressed, persecuted, and, as he imagines, brought to ruin. A third, as Dalmas and many more, slay or attempt to slay out of vindictive feelings from supposed injury. A great part of the Scottish nation, possessors of the *perfervidum ingenium*, might be said in former times to suffer under this species of derangement; for when two parties, one of whom believed he had sustained an injury, or received an affront from the other, met, it seldom happened that they parted without one shooting or stabbing the other. The Celts were still more remarkable for dirking and every sort of killing in very slight causes, so slight, indeed, that it would quite perplex a modern jury to discover what they often search for, namely, a motive or inducement.

Another difficulty in these cases is the existence and influence of intermittent or periodical insanity. We have seen various instances of persons conducting themselves perfectly well,—calmly, steadily, and properly, for considerable spaces of time, that is days, or weeks, or even months. But all of a sudden, from some very slight cause, or without any perceptible cause at all, these same persons become violent, outrageous, and unmanageable, discharged at those disagreeable to them whatever was within reach; broke and destroyed articles often of great value, and it might be, attempted to kill the persons who were nearest them. After all this, calmness again returns. To the subjects of these cases, when they have committed an act involving a breach of the laws, it is difficult to apply the ordinary rules. The individual was insane when he committed the act; but he is sane when brought to the bar.

We are however getting into a psychological discourse when we ought to be informing the reader of the doctrines of Dr Mayo. As we doubt not, that they will prefer to our observations the views of Dr Mayo, we subjoin a chapter which embodies the principal part of the inferences which he forms.

"A child is born in the garrets of St Giles's, the offspring of a prostitute and a thief. He receives a suitable education, and he adopts the father's profession. After following it with assiduity and success, he is arrested in the course of it, and tried for his life. His early habits are ascertained on unquestionable evidence; but do these considerations soften the rigour of the law, by placing him before the public as an irresponsible being, the creature of circumstances over which he has had no control? By no means. In one respect they tend to ensure his condemnation; namely, in making his deliberate performance of the criminal act more probable. Finally, this person is hanged or transported according to the nature of his offence. And it is, probably, very fit that he should be so treated. I do not object to the decision of the law, in the case which I am supposing, provided a consistent view is taken of certain other cases, in their relation to the plea of irresponsibility.

"It is clear we do not test the liability to punishment of the above offender by his presumed moral responsibleness, but by some other criterion; and this criterion, I will at once assume, is furnished by the question, whether punishment, in the case supposed, will or will not prevent the repetition of the offence.

"Now the above remarks have an important bearing on the subject of the unsound in mind, viewed in its relation to delinquency. Their exculpation (when allowed) at present rests on the plea that they are irresponsible; but this plea is not permitted to operate in favour of the case first supposed, to which it is applicable in as high a degree. Yet right reason and the sympathies of our nature appear to dictate a common criterion in common cases.

"The tendency, which has unconsciously operated on the public mind, to disregard the community of principle on which the sane and the insane may in many respects be dealt with, had largely contributed to that moral mismanagement of the insane, which many wise and humane persons are, in the present day, endeavouring to obviate in the regulation of establishments for their relief. It is now seen that they can be influenced in a high degree by a sense of right, by fear of consequences, by gratitude, and by a well-modified love of praise.

"What, then, are those states of the unsound in mind, for which and under which they must be held unamenable to punishment for offences against society, on the ground that their punishment will not prevent similar offences in themselves or others?

"In the case Rex *v*. Offord (Trial of Macnaughten for the murder of Mr Drummond), the prisoner was indicted for the attempt to murder a person of the name of Chisnall, by shooting him with a gun. It appeared that the prisoner laboured under a notion that the inhabitants of Hadleigh, and particularly Chisnall, the deceased, were continually issuing warrants against him for the privation of his liberty and life. Several medical witnesses deposed to their belief, that from the evidence which they had heard, the prisoner laboured under that species of insanity called monomania, and that he committed the act while labouring under the influence of this disorder. Lord Lyndhurst, who tried the cause, in summing up, told the jury, that they must be satisfied, 'before they could acquit the prisoner, that he did not know, when he committed the act, what its effects, if fatal, would be in reference to the crime of murder.' Now, the converse of this proposition, namely, that such ignorance as is there supposed of the consequences of the act in reference to law, ought to avail, as exculpatory of the delinquent, is equally consonant with our sympathies, and agreeable to right reason.

"The punishment of an unsound person, whose disorder of mind has proceeded to this point, can in no degree answer the intention of punishment. It cannot prevent a repetition of the offence, for it will not be appreciated by the persons whom it is supposed to influence. Meanwhile this description will embrace all the mass of cases in which delirium is present; cases well recognized in practice, though not easily fixed by definition. Again, it will embrace all such acts as, springing out of an insane delusion, though not performed under delirium, would be justifiable, if that delusive impression were founded on a reality. Thus, a person insanely imagining his own life the object of a conspiracy, and killing another person, under circumstances which the law would warrant as constituting an act of self-defence, if the conspiracy were real, would be protected by the above plea against penal consequences. The prior delusion of the offender may, in this instance, involve the commission of an act, under ignorance of its criminal nature, that is, of its effect with reference to the crime of murder. I will put the case thus:—A considers himself the object of a conspiracy, and considers B one of the conspirators. He meets B in a solitary place by night; his fears are aroused under the prior delusion, and he puts B to death in presumed self-defence.

"In these two suppositions,—namely, that of a delirium present at the commission of the delinquency, and that of temporary misconception of the nature of the act in relation to law,—we comprehend as unamenable to punishment, both the maniac generally, and the monomaniac, so far as he has a right to this immunity on the principles laid down. But eminent authors, such as M. Georget and Dr Prichard, are of opinion that the plea of insanity may comprehend monomaniacs, so as to render them generally irresponsible, or unfit for punishment. Now, I entirely assent to Dr Prichard's opinion, that perfect soundness in other points is not compatible with the presence of monomania. But I do not imagine that the law either does contemplate, or ought to contemplate this, as conferring on them general irresponsibility. Surely the public interest requires that a person should be amenable to penal law for the consequences of those aberrations for which he cannot legally be coerced or confined as insane; and such is generally the position of the monomaniac, at least for a long period of his morbid state. He is indeed rarely made the subject of restraint, until he has committed some act which the fear of punishment might have prevented.

" The plea advanced in general exculpation of the monomaniac, on the ground of justice, is disposed of by the precedent with which this essay commences. For the person who can reason upon his crime, —who can devise systematic means of accomplishing it,—who can form and weigh opinions respecting his chances of escape,—can conduct the ordinary affairs of life, and take care of his property, cannot, consistently with justice, be made less responsible for his actions, than the unhappy offspring of the prostitute and thief, whom we there suppose. With respect to the other criterion, the question, namely, whether his punishment will prove preventive, the answer to that question is embodied in the well-known reply of a monomaniac in imprisonment, to a friend, desiring to know, whether he thought that the York incendiary was likely to be hanged? 'No,' says the skilful calculator of chances, 'he is one of us.'

" Let us now consider, in relation to these principles, the important case which has given occasion to the public anxiety on the whole subject: I mean the case of Macnaughten. In the first place, no proof was offered, or I presume could have been offered, that he was not cognizant of the nature of his act in relation to the laws. Neither, allowing him the benefit of a pre-existent delusion, could he have construed the circumstances under which he performed the fatal act as affording him the plea of self-defence under an immediate attack. These grounds excluded, what other grounds were there for supposing that his punishment might not deter similar offenders, or that the apprehension of punishment might not have deterred him? Was his insanity in relation to the act committed analogous to that of the regicide Hadfield, or to that of the mother who murders the offspring dearest to the heart? Did it subvert or run counter to the ordinary workings of the human mind? If, as I believe to have been the case, he was and is a monomaniac, he was not therefore, *prima facie*, incapable of learning, or of teaching others by the example of his punishment, that a strong desire of vengeance, or, if you please, of prevention of mischief, must not be gratified in defiance to the laws of his country. To say that he was incapable of making this distinction would be to assign him a pathological state of which no evidence was adduced; he must have been a maniac, not a monomaniac.

" I have endeavoured in the above reasoning to obtain points of distinction which may enable us to call into action for the defence of mankind, that power which the insane are known to possess under certain limitations, of controlling from prudential or other motives their own tendencies; that power which, under the improved management of the present age, is so often employed for their benefit, and sometimes for their cure; and which in these instances might be exhibited in so far limiting their delusions, as to render them compatible with the peace of society. Occasional difficulties must arise in regard to this as well as every other class of offenders, in applying penal principles. General rules must occasionally apply a pressure too stringent at some point; but in this case it must be remembered that the interests of the monomaniacal are as much concerned as those of the public. It is, indeed, a most oppressive infliction, though one by no means preventive in its character, since it does not come in a penal shape, which these persons incur under our present system. We wait, for instance, until they have committed some murderous act, and then shut them up in a madhouse for life. Had the friends of Macnaughten viewed him as liable to a capital punishment, in case of an outbreak against society, of which in kind he seems to have given them ample notice, it is reasonable to presume that some means of cure or prevention might have been resorted to by them.

" In Macnaughten's character, murder was probably in some sort the crowning point or climax, as Dr Monro aptly termed it, to which his delusion tended. In a milder or more kindly character the blood of the offending party would not have been pointed to as requisite, or the requisition would not have been listened to. Again, had insanity occupied the whole character and conduct of Macnaughten, he would have afforded no preventive example to others by his punishment, and its infliction would have been gratuitous. Such as he was, his punishment would have afforded an invaluable example to thousands of ill-regulated minds, in which a seed of insanity springs to life in a hot-bed of evil passions, or wayward dispositions, or overstimulated imagination; while the impunity accorded to the name of mental unsoundness places at the disposal of their passions the means of gratification. It is in this way that Sir Walter Scott exhibits the effect of a monomaniacal delusion on the feverish and impassioned character of Allan Macaulay—(Legend of Montrose.)

" To the argument in favour of the punishment of the partially insane, on the ground of its preventive effect, it has been answered, that the insane are, as such, unaware of their own state, and consequently will not be influenced in this way by the escape or punishment of other insane persons. And this reply would have some weight, if its assumption were generally correct that they contemplate the others as insane. But this assumption is erroneous. Those whom they recognize as actuated by motives and views similar to their own, they regard equally with themselves as of sound mind, and only (as an insane lady once described herself to me, when I pointed out the deviations of her conduct) ' in a minority.'

"In the 20th chapter, I adduce the case of a gentleman availing himself of the privilege of a well-marked hallucination, to shelter himself from the effects of some very disorderly and disgraceful conduct, of which he had been guilty, quite unconnected with it. He was ingenious enough to rescue himself in this way from the police authorities in a French town, in which he was staying. This gentleman by no means considered himself as insane; but had learnt that certain habitudes of his mind would be accepted as exculpatory of his vices.

"The cases, indeed, are numerous in which our present theory of monomania either has exculpated, or might have exculpated, most erroneously. In the case of Bellingham affidavits were put in, stating, that if more time had been given before trial, evidence establishing some features of monomania would have been obtained. In this case Bellingham would have escaped punishment, though, on the grounds which we have maintained in respect to Macnaughten, he obviously deserved it.

"In the murder of Mr and Mrs Bonar of Chiselhurst, the confession of Nicholson, evidently made with no purpose of trumping up a story of lunacy, bore strongly the character of an attack of homicidal monomania: and had not this supposition depended entirely upon a statement made by himself, it is very probable that circumstances confirmatory of that supposition might have been sought out. In this case, by favour of that view of the subject which I am endeavouring to controvert, this murderous villain, if assisted by able counsel, would probably have escaped into a madhouse, and thus an important preventive example have been removed from the public eye.

"There were points of eccentricity in the character of Lord Ferrars, which in the present day, in which Captain Johnson has escaped, would have made *his* chance a very good one. Lord Ferrars was in truth of unsound mind: but his unsoundness belonged to a species of that great genus, which ought to confer no immunity from punishment. He was eminently and exquisitely a brutal character, as contradistinguished by me from the insane character in another work,—(Elements of the Pathology of the Human Mind,) by T. Mayo, M. D., to which I must refer my reader. He was a man without a moral sense; fear could alone have controlled him: and it was evident that his intellectual powers were sufficiently vigorous and clear to have estimated the nature and chances of capital punishment, and to have contrasted it with the pleasure of gratified revenge.

"Macnaughten himself expressed in a conversation between him and the medical visitors, which one of them, Mr Maclure, reported at the Harveian Society, that 'he should not have liked to be hanged for his attempt on Mr Drummond's life,' but he added, that 'he felt sure the Duke of Wellington would not hang him.' In fact, the strong indisposition evidenced in the present day to inflict capital punishment gives to many offenders a dangerous reliance on escape.

"Some very mischievous escapes have taken place under the vagueness of our law in its application to this subject. It has, indeed, been penal or exculpatory, just as the case might chance to influence the sensibilities or the reason of the jury or magistrate. The impunity of that officer who made an attempt on the life of Lord Palmerston is one of these cases. 'And such also was the case of Miss Broadrick, who murdered Mr Errington. In drawing a distinction between her case and that of Hadfield, Mr Erskine very justly argued that Miss Broadrick acted in reference to circumstances which had a real existence, as well as under the influence of those motives and passions which are naturally excited by such circumstances. He saw no connection between her case and insanity; and was of opinion that she ought not to have been acquitted. In the Annual Register of 1795, in the report of that case, Chief Baron Macdonald, before whom it was tried, is noticed as having considered the laughter of Miss Broadrick after the commission of the horrid act, as an evidence of insanity, and to have charged the jury to that effect. 'Moody madness' is, indeed, described by the poet as 'laughing wild amid severest woe;' but the Chief Baron should have considered that these latter expressions ill describe the state of feeling produced in a violent and injured person by gratified revenge. Miss Broadrick had punished the man by whom she had been deserted, in the presence of her rival—a very sane procedure, though a very shocking one.

"Assuredly the case of Oxford would find also a place in the above number. This young man was plainly capable of appreciating his own offence in relation to the laws of the country, and his general knowledge of right and wrong was equal to that of the felon whose case we supposed at the commencement. His punishment would have acted beneficially on the fears of others. Whether in favour of the weak-minded, whose acts are not characterized by sufficient deliberation to entitle them to the awful punishment, some other punishment might not be deemed a sufficient preventive, is a question which has already met with some legislation. The entire escape of Oxford was confessedly most mischievous.

"That the extracting grounds and motives for penal measures out of a theory of desert and moral responsibility, or, in other words, of abstract justice, is unattainable by human legislation in any class of offences, has been already noticed. But this matter deserves to be reconsidered. Justice, in its sense of securing to every man his due, may be considered in two relations; either as regulating the dealings of mankind and their possession of property, or as correcting the offences which they commit against each other. In the first of these relations, the theory of justice, the giving every man his due, is aimed at, and, humanly speaking, accomplished by law. In its second relation, with the present endowments of the human mind, the realization of justice is not equally easy. Some aids it no doubt possesses in the construction of our minds. The feeling of indignant emotion was, no doubt, imparted to us, as a guide towards punishment viewed as an act of justice. Still the appreciation of the grounds on which, and the extent to which that feeling may act, is vague, indeed, in comparison with the precision with which the contracts and exchanges of distributive justice are carried on. Man was made to live with his neighbour on terms of justice: but when he is compelled to punish his neighbour, unless he could read the heart of man, he must be contented with another ground-work for his proceedings, namely, the principle of prevention. Still, in taking this ground, we anxiously crave as near an approximation as possible to that of justice, and desire that, although prevention must be the immediate motive of penal inflictions, a theory of desert should be the measure of their severity; insomuch that the preventive force of punishment will be greatly increased, in proportion to the extent to which the admeasurement of the punishment gratifies this moral appetite, and avoids the semblance of injustice. Indeed, unless this precaution is used in the admeasurement of punishment, it will become a dead letter. The sympathies of mankind will resist its enforcement. I am, indeed, far from advocating severity of punishment. I am merely demanding that the consequences of social offences committed by the insane should not, as at present, be wasted in unproductive and unreformatory suffering, that the madhouse should, in certain cases, be changed to the gaol. At present, it has often all the attributes of a most bitter punishment, except that which tends to prevent the repetition of the offence. If thought of by others, the incarcerated person is, for the most part, only contemplated as one who has plenty to eat and drink, and nothing to do. The grounds on which Macnaughten was acquitted as a felon, were insufficient; it does not follow that the punishment appropriate to his case was a capital one.

"No doubt, difficulty must be felt in applying the limitations and distinctions in punishment which this view suggests. But is not this difficulty connected in a degree with our judicial arrangements, which leave the subtle questions of mental unsoundness at the disposition of juries. As the matter stands at present, the great, the good, the high-placed, who in this country are likely also to belong to the two first classes, are placed, under the influence of circumstances over which they have no control, in direct exposure to the knife of privileged assassins; privileged, indeed, in two ways, for they enjoy the immunities of presumed sanity before they commit the crime, and of presumed insanity after its commission.

"Before I quit this subject, I may make some remarks upon a part of it which immediately concerns the interests of the medical profession, but which has also great public interest. It constantly occurs to us to find our opinions solicited and implicitly accepted, not only in our proper province, the pathology of such cases, but also as to the application of the penalties of the law to the acts which spring out of it. Thus, the Chief Justice stopped the trial of Macnaughten, because all the medical witnesses concurred in opinion (not only that he was a monomaniac, which was the point to the extent of which they ought to have been consulted, but) that he was 'an irresponsible agent,'—by which they meant a person not liable to punishment; certainly a very distinct consideration. Now, defective as our law is, had the Chief Justice tested the opinions of these medical witnesses by the legal criterion offered in the opening speech of the Attorney-General, he would have probably replaced the medical witnesses in their right position, that of testifying to the madness or sanity of the patient, not to his guilt or innocence. But he was contented to accept from them law as well as medicine.

"The province of medical advisers in the above point may be clearly defined. It is their duty to expose any mental unsoundness which they may discover in the presumed delinquent, and to afford such information as may enable judge and jury to appreciate its influence in rendering punishment inapplicable, or leaving it applicable according to the principles of the law. In the case of Macnaughten they had to suggest grounds for determining whether, at the moment of his muderous act, he was aware of its fatal and of its criminal nature, or the reverse. Thus dealt with, a large field of thought and investigation would be left to the medical adviser; and medical evidence, in this class of cases, would become a safeguard to the public as well as to the delinquent.

"In making these remarks, I disclaim any tendency to depreciate evidence given by medical witnesses in this class of cases. In their general reference to our profession, the insane are objects of tenderness and of curative treatment. It cannot be a matter of surprise that we should be averse to consign to punishment those whom we are habituated to regard in this kindly relation. As witnesses in a trial, we are in the hands of the court, and the error does not rest with us if we are desired or permitted to depose to points which do not properly fall within our province.

"But it is also urgently wanted, that the *public mind* should be instructed on this important subject. I believe that the law, though requiring explanation and commentary, is nearly in substance and intention conformable with the above reasoning. It does not intend to admit the plea of insanity, except where there is a proved state of mental unsoundness at the moment of commission, and in relation to the offence. Yet judge, jury, and advocates have, even since the case of Macnaughten, concurred in the acquittal of Dalmas,* who deliberately murdered a female on Battersea bridge; and in that of Captain Johnson, who slaughtered half his crew;—of the one, under the presumption that his head had suffered some former injury; of the other, with no reason at all except a vague impression of mental unsoundness, of which no medical proof was given at his trial. The supposition with which I opened these remarks, is at the bottom of this error, or this opinion if it be presumptuous to call it an error. The powerlessness of the maniac, under most circumstances, to exert self-control is striking, and it is within the commonest perceptions. The partially insane, who doubtless have common points with him, participate unduly in the prestige, which justly operates in his favour; and thus they obtain an extent of impunity which they have no more right to than unnumbered delinquents, who incur the highest penalties of the law. And it is perhaps fortunate, in some respects, that this ground exists for rhetorical appeals to our sympathies; for, viewed irrespectively of these affecting topics, the insane are certainly not objects of keen sympathy. Men do not readily go along with them. We can understand and enter into the distresses of the vicious (provided there is no shocking enormity in their offences), because their sorrows and pleasures are our own *in kind,* though we know how to avoid the pleasures which are immoral, and the sorrows which are unreasonable. But we do not understand the smiles or the tears of the maniac: we are not in unison with him.* Thus, the circumstances which place mankind in temporary sympathy with them, may have their advantage in ensuring to them kindly treatment; but this advantage is purchased dearly, when we are induced, by the same train of emotions, to extend the excuse of insanity to crime. —Pp. 193—209.

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* The sanity of Dalmas has since been made clear, and he has been transported.

These observations state and explain the conditions, in which is placed the question regarding the admissibility of the allegation of insanity, as an exculpatory plea, in cases of the commission of acts allowed by all mankind to be criminal, and of wrongs and injuries inflicted on others. They show how persons born and bred in regions of wickedness, and breathing an atmosphere of vice, may have the moral perception so obliterated, as not to distinguish right from wrong, or good from wicked actions; and to go on in the habitual commission of deeds which violate every law of God and man, without showing that they are aware of the fact in any other way, than to avoid detection, and evade punishment. With these persons the question is not the nature of the act, but whether there is a chance of being discovered and punished. Now, this is not exactly the case of the madman or insane person. He never thinks, in the commission of a criminal act, whether it is to be discovered and punished or not; and he rather courts exposure, and has no dread of punishment. It is true that Martha Brixey did ask the question beforehand whether women were ever hanged; but the act of asking the question seems in this case itself to show, that she was deranged, or at least very ill informed. The act which the madman commits he thinks not criminal at all; and all his thoughts are absorbed in the single point of slaying the person or persons whom he imagines he ought to slay, in removing him out of the way, in avenging a supposed insult or injury, or simply in gratifying his desires without any very distinct reason.

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* This fact will be admitted by all those who do not study insanity in novels and tragedies.

The observations of Dr Mayo do not therefore furnish us with any key to the solution of the difficulties, in which this question of the circumstances and conditions, which justify the admissibility of the plea of insanity in criminal acts, is placed. They are much in the same predicament as the sermons of Calvinistic divines and Methodist preachers, which dwell incessantly on the wickedness of the human heart, and the total corruption of the whole human race. But what is wanted is some psychological or judicial test or criterion, by which we could at once determine how much insanity and how much wickedness enters into the commission of every criminal act, or admitted wrong to others.

This test these observations do not furnish; and we are not surprised that they do not. This alleged corruption of the human heart we do not deny. Its daily effects are too obvious to allow this to be questioned. But though this be admitted; though it be allowed, that man is a being base and frantic, that does not remove the difficulty. What we wish to know is, how much of every immoral and illegal act depends on that obscuration of the intellectual and moral faculties which is called derangement, on that confusion and disorder in reasoning, which persons partially insane evince, and which leads them, as it were by impulse, some call it blind, others term it uncontrollable, to the commission of acts, which all mankind agree to regard as guilty. This is the great difficulty, and one which we fear neither philosophers, mad-doctors, nor judges, will solve in a short time. Yet all these orders of speculators are ready to furnish what they conceive to be the test. They tell us it is the motive, the intention with which the act is done, which constitutes the crime; then we are reminded of the moral responsibility of the party accused. This moral responsibility, however, is the very thing sought for; and in discovering it, not one person agrees with another. On Wednesday, the 15th of December 1847, a young boy of 12, accused of poisoning his grandfather, was tried at the central criminal court and convicted. A strenuous effort was made by the counsel for the prisoner, with the aid of medical witnesses, to prove that he was insane. It seems to have escaped the notice of these learned persons, that to administer poison so as to cause death, requires a certain amount of plan, foresight, and deliberation, all of which seem not consistent with the confusion of mental derangement. The judge heard all parties patiently, and stated that he could see in the facts adduced, no evidence of the insanity of the prisoner, and expressed this opinion distinctly to the jury, who returned a verdict of guilty; but with a recommendation to mercy, on account of the youth of the prisoner. Whether this boy suffers capital punishment or not, it is clear that great difficulty was observed in defining the limits between insanity and criminality. There was no doubt that the old gentleman was poisoned; there was equally little doubt that the poison was administered by the grandson; and the question to be determined was, whether he did so with the intention of killing his grandfather, or without being aware that the act was to be followed by the death of the old gentleman?*

We cannot agree with Dr Mayo, in thinking that the act of Miss Broadrick was that of a sane person. It was the act of a woman deceived, betrayed, incensed, and vindictive; and with these feelings sanity is quite inconsistent. It was natural that a female so badly used should take vengeance on the person to whom she ascribed the injury; but, on the one hand, we are taught, that we are not to avenge ourselves and our own wrongs; and on the other, were every one to adopt this wild system of taking justice in his or her own hands, it would cause complete anarchy, confusion, and disorder, and convert society into a scene of endless warfare and bloodshed, with greatly more injustice than it proposed to remove. Every woman also, who has been injured in the manner Miss Broadrick was, is to a certain extent insane from various causes. She is disappointed in her expectations; she has lost caste; the finger of scorn and the tongue of scandal are both levelled at her; her pride is mortally wounded; and worse than all, she is stung with feelings of shame and remorse; and how can it be said, that a woman labouring under these opposing feelings is not insane? It is a mere question of opportunity, whether she is to commit suicide, or assassinate him to whom she ascribes her sufferings and her degradation.

ART. VII.—*Speech of the Count De Montalembert in the House of Peers of France, the 5th June* 1847, *on the subject of Medical Reform; with Notes of the Translator.*

THIS translation of an excellent speech has come to hand barely in time for a very brief notice. The subject of Medical Reform is agitated on both sides of the Channel, but under very different circumstances. Here the demand for a more liberal legislation has proceeded from the profession itself, and the Legislature has done literally nothing towards effecting the object, owing to the difficulties with which all such attempts are embarrassed by the opposition of privileged medical incorporations. In France, on the other hand, the question of medical reform is in the hands of the Administration, who have introduced a series of bills relating to this most important subject. Of these bills the Count De Montalembert appears as the warm and decided opponent, on grounds which will be made intelligible to our readers by the production of the following extracts from his speech.

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* See Note in p. 240.

"The manifest intention and prevailing object of all these bills is to extend the influence of the Government, not only over public education, where its unchecked despotism is already but too well established, but over the mind, the thought, and the science of the nation."

"I think the Government is hurrying to its destruction, in thus maintaining and declaring itself the successor and heir of the great feudal system, that is to say, of an excessive, corrupt, useless, minute, and vexatious power. . . . But the feudal system, in its day, was a splendid and powerful institution, as well as a real social benefit. All historians, all philosophers, are agreed on this point. But this political combination received its death-blow when it sought to invade, to govern, to meddle everywhere, and to make all men feel the yoke of an incompetent government. So, also, in our days, a powerful government has been found a great advantage, so long as it was necessary to act against the abuses of a preceding *regime;* but if we continue to pursue the course in which the present government would push us, I am convinced that we also shall become the objects of hatred and insurrection."

After comparing "political society and civil life" in France at present, to the course of the Rhine in feudal times, beset with tolls, barriers, castles, and extortion, he proceeds thus—

"At every turn we are stopped and required to pay an impost, to exhibit a license. If our vocation be to teach, we must ask leave to do so; if we desire to publish our thoughts, we must not only be qualified by education, but licensed by the Government to do so; if we discover a well of mineral water on our property, we must not give it to our friends to drink without the permission of the police; and by this new bill, which is a great aggravation of all our former ills, it will be in future a crime punishable in the heaviest manner, to give charitable advice, or dress the wounds of our neighbours, without the authority of the Government."

While this liberal peer thus denounces the Government for its busy, meddling, patronizing disposition,—for declaring medicine to be "a priesthood," and physicians to be "officers of State," he is equally indignant at the position taken in relation to this question by the members of the medical faculty itself. In opposing the bill, they appear to have displayed "a corporate feeling, the reasonings of a caste, the selfish, almost greedy, demonstrations of a trade;" which, as he says, have altogether shocked him, and which appear to him to deserve the ridicule of Moliere and of Le Sage as their only appropriate answer. What the precise nature of their objections to the bill may be is not stated; but it appears that they agree with the Government in regarding the profession as "a sacerdotal office, a kind of magistracy, or a public function to be viewed with some particular veneration;" while he, on the contrary, holds medicine to be "a liberal art, freely exercised, and freely accepted by ourselves and our families;" and the office of a physician to be one which has "nothing to do with the State."

Our limits will not admit of our transferring to our pages a specimen of this French nobleman's eloquent exposure of the follies and persecutions perpetrated by medical men in their days of privilege, and of their futile endeavours to repress useful innovations on established maxims. For these we must refer our readers to the work itself.

On the important subject of penalties, privileges, and ranks in the profession, the Count gives a very clear and decided judgment. He "would have classes among medical men, but these classes should be an honour, a recommendation, a guarantee of capacity, and a title to public confidence, but neither an arm of offence, nor a means of oppression against those who are unable or unwilling to take so distinguished a title." "Let physicians and surgeons have their titles placed under the protection of the laws, and severely forbidden to those who would usurp them. But to compel all those who would offer advice in matters of health to assume such titles is a most unnecessary encroachment on individual liberty, and a coarse limitation of the freedom of the human mind."

On penalties he reasons as follows:—"I not only admit them, I claim them, but, in the spirit of our laws. I ask even severe penalties; but against what? Only against the illegal assumption of titles. Should any one offer advice, and assume a name which the law has not conferred on him, let him be severely and surely punished. But that the simplest and most easily understood offices of a physician or surgeon, when practised by either men or women, without imprudence or ignorance, should be punished as a misdemeanour or felony by severe enactments—this is what I do not understand, and shall never admit to be either reasonable or just. I do not then allow any other penalties than such as are applicable to the usurpation of a professional title, or to such abuse of a professional position as would argue imprudence, unskilfulness, or immorality on the part of a physician."

The Count contrasts the state of medical legislation in France with that in Great Britain; too favourably, as we think, for the latter, but with the good purpose of showing that our comparative exemption from the restrictions which he condemns, is not attended with any less efficiency of the medical profession for the purposes for which it exists.

On the subject of quacks and quackery he declares, that the titled and legal quacks inflict the greatest injury on the profession in the judgment of physicians themselves; that quacks always have existed, and always will, in all professions; that if they have talent they will cease to be quacks, and, if not, will cease to be dangerous; and that the good sense of the people, if public opinion were less cribbed and confined, would be a better safeguard than the authority and power of a government. He complains of its being a settled maxim that the human race is deemed "unfit to manage its smallest and most customary interests, while the little and pitiful minority of appointed agents of the Government are assumed to be infallible." He says that he cannot discover "where we are to stop in such a career, for there is no profession, no occupation, in which there are not to be found men able and very willing to take advantage of public credulity;" and that the Government in this way will never be without a plausible excuse for "substituting its own meddling, and oftentimes bungling, for the safe and wholesome direction of public opinion." He contends that the Government ought, in consistency, to regulate all other professions as well as physic, and to prevent you even from hiring a governess who has not previously received a public sanction, lest she should impose on your credulity.

We have thus endeavoured to give our readers some idea of the views of this distinguished foreigner, as developed in his very able speech. It is one of those speeches which, recurring to first principles in legislation, is in its scope applicable to the institutions of other countries than his, and will find an echo among the liberal and reflecting part of the profession and of the public in this country; especially in its great leading arguments that medical legislation ought to be based exclusively on the interests of the public, and that the public are better judges than public functionaries are, or can be, of the fitness of medical men, licensed or unlicensed, to be intrusted with the care of their health. The views which Montalembert has expressed as to the inexpediency of the prosecution of quacks and unlicensed practitioners, unless for actual misconduct, immorality, or assumption of titles which do not belong to them, are not new, though they are pithily and powerfully expressed. We could easily indicate many expositions of the same views by enlightened parties in this country. We would particularly mention Mr Warburton, the chairman of a committee of the House of Commons which took evidence on this subject many years ago, as a gentleman who has long held and ably maintained the same opinions.* In this end of the island these opinions are almost universal among medical men;* and they are not merely theoretically held, but practically acted on, by the abandonment of prosecutions for many years by all the privileged boards in Scotland.

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* See Report by the Royal College of Surgeons of Edinburgh, in our 55th vol. page 270. 1841.

We have mentioned that Montalembert has contrasted the restrictive system of his own country with the greater freedom of the profession in Britain. We would receive this eulogy of a distinguished foreigner with becoming modesty, and with a deep sense of the extent to which our own institutions would require to be amended to make them deserving of the commendation of one whose views are so liberal and enlightened. We agree with him most thoroughly in his general principles, and are not sure that we differ from him even in matters of detail, if his views were more fully explained; excepting indeed on one point. It seems to be his opinion that we might with propriety and safety confer medical qualifications on the mere strength of examinations properly conducted, without a curriculum being made a necessary condition. After much reflection, we differ from him on this matter. We think that our system of examinations for medical qualifications admits of much improvement, but that it cannot be made a perfect criterion ; and that a course of study under capable teachers is a most important security for the efficiency of medical instruction, which it would be most unwise to relinquish. This appears also to be the opinion of the translator.

Montalembert is very much in earnest about what he terms the *freedom of teaching.* If by this he means, as he probably does, that he is opposed to all monopoly in this most important department, we do most cordially and thoroughly agree with him. Teaching is, in all departments of human knowledge, one of those great highways of ambition and emulation from which the public at large is greatly interested in removing all needless impediments. Every man ought to be permitted to teach others that which he knows. But it does not follow that all teachers ought to be admitted to the same status. It is enough that it is open to all to acquire that status for which their talents and acquirements have fitted them. If a course of instructions is to be required by public authority, as a condition of obtaining medical titles and qualifications, it would be altogether monstrous if these instructions might be given by those who possess no qualifications, and have given no public proof of fitness to teach. No capable teacher can be at any loss to give such proof, nor can it be wrong or illiberal to require, as a condition of his status being admitted, that this proof should be given by him, or that he should be amenable to just conditions and regulations calculated to secure the efficiency of his instructions. The tendency of opinion in this country, and especially in this part of the country, is towards greater strictness in conferring the status of a public teacher of medical science. We feel assured that this is right in itself, and perfectly consistent with liberality ; and we are not at all sure that the Count woul differ from us in this, though some of his expressions may seem, on a cursory perusal, to be inconsistent with it.

On the whole, we welcome this speech as a valuable contribution to the liberalization of medical institutions, and consider the profession and the public at large to be indebted to Montalembert and to his translator. As far as we can judge, the latter has conveyed the spirit of the original in pure and excellent English, free from idiomatic peculiarities of the original language in which it was spoken. We hope it will serve as a warning to ministers of State and legislators not to seek to pervert a learned profession into an instrument of political influence; nor, in avoiding this error, to slide into another, which is far more likely to happen in this country, or rather which has already happened to a great extent, and which it must be the business of legislators to cure, that of surrendering the profession to be mismanaged by certain great medical incorporations for their own selfish and ambitious purposes. It is a disgrace to our Legislature that they have made no progress hitherto in the cure of evils of the nature of which they are or ought to be fully aware. Far be it from us to desire the extinction of our medical colleges; but our legislators will never make any progress in the cure of the intolerable anomalies and grievances of medical legislation, if they do not resolve to turn a deaf ear to all remonstrances proceeding from such bodies which are founded, as they too frequently are, on selfish and exclusive views, and if they do not base their measures on public interests alone, to which the whole machinery of medical schools, colleges, and incorporations ought, in all reason, to be altogether subordinate and subservient.

ART. VIII.—1. *Elements of Chemistry, including the Actual State and Prevalent Doctrines of that Science.* By the late EDWARD TURNER, M. D., F. R. S. L. & E. Eighth Edition. Edited by BARON LIEBIG, Professor of Chemistry in the University of Giessen, and WILLIAW GREGORY, M. D., F. R. S. E., Professor of Chemistry in the University of Edinburgh, Part I. Inorganic Chemistry. London, 1847. Part II. Organic Chemistry. London, 1847. 8vo. Pp. 1394.

2. *A Manual of Elementary Chemistry, Theoretical and Practical.* By GEORGE FOWNES, F. R. S., Professor of Practical Chemistry in University College, London. With numerous Wood Engravings. Second Edition. London, 1848. 12mo. Pp. 596.

WE have so often had occasion, in different volumes of this Journal, to speak in terms of approbation of the Elements of Chemistry by the late Dr Edward Turner, that it cannot appear wonderful, that we direct the attention of those interested in the progress of chemical science, and its applications to medicine, to the present enlarged and improved edition of that useful treatise.

In 1848, the Seventh Edition of these Elements was published, after a careful revision, and with the introduction of the requisite corrections and improvements, by Professor Liebig and Dr William Gregory.* The changes and additions, especially in organic chemistry, were considerable, both in magnitude and number. But they were done with ability by these two eminent chemists.

Since that period, the progress of chemistry, the discovery of various new substances, and more perfect knowledge of the properties and actions of others previously known, new and more accurate views and methods of explaining various chemical phenomena and processes, and, in short, a large amount of additional information, have rendered the task of preparing another edition at once more necessary and more laborious.

The eighth edition, accordingly, appears in an enlarged form, and presents throughout internal evidence, that it has undergone most careful revision in the two great departments of Inorganic and Organic Chemistry. The former division presents the smallest proportion of changes, additions, and remodellings. Yet even here the hand of renovation and correction is manifest. In the second part, devoted to organic chemistry, this is still more manifest. In point of fact, the second part has been nearly written anew, in order to embrace all the new discoveries and new views of chemical phenomena, and to give a proper and adequate representation of the present state of knowledge as to the chemistry of vegetable and animal bodies. We know that it is difficult for any but professed chemists to form a just estimate of the amount of labour bestowed on a work of this kind. But while we see the rapid progress of chemical research in the department of organic chemistry; when we know how difficult it is to collect from all the different sources in which they are scattered, the new information daily and weekly added to the general store; and when we peruse this work, as we have done, and compare the information which it contains with that which is contributed by the chemists of Europe, we can safely say that the editors have succeeded most completely in giving a correct view of the present state of chemical science, its leading doctrines, and their application to physiology and medicine. It is evident that one of the editors, we think Dr Gregory, has been most anxious to give at once the most recent and the most correct views on organic chemistry; and we think he has shown great judgment, as well as accurate knowledge, in the accomplishment of the duty which he has undertaken. The present is, in short, the most complete and the most luminous system of chemistry in the English language; and we know no one in France or Germany that comes near it.

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* Elements of Chemistry, including the Actual State and Prevalent Doctrines of that Science. By the late Edward Turner, M. D., F. R. S. L. and E. Seventh Edition. Edited by Justus Liebig, M. D., &c. and William Gregory, M. D. &c. London, 1840.

The Manual of Elementary Chemistry by Mr Fownes is, of course, a work on a much more limited scale, and cannot be placed in the same rank with the Elements of Dr Turner. It is indeed a manual, as its name implies. But it is a very good manual; presents shortly all the leading facts and doctrines, and gives as ample a view of chemical science as it is possible to do within compass so limited. As a pocket, or laboratory, or class-room companion to the student, it deserves every recommendation. This appears, so far as we can judge, to have been the object of the author's ambition; and that object he has done everything to attain.

NOTE ON THE CASE OF WILLIAM NEWTON ALLNUT, REFERRED TO IN PAGE 232.

The grounds on which it was attempted to be proved that William Newton Allnut was insane, were the following:—1st, His father had been subject to epileptic fits, used to drink immoderately during the two years preceding his death, and died in a complete state of madness at the age of 37. 2nd, His mother, when pregnant, and at the time of the birth of the boy, was in great trouble, and her mind was considerably affected 3rd, The boy, when young, fell on a ploughshare, and the effects of the injury were said to have affected his mind. 4th, He was scrofulous, and had ringworm, and used to walk in his sleep. 5th, His misconduct caused to his mother great trouble and distress, more than she had with any of her other children. 6th, One medical witness, Dr Conolly, stated that he was imperfectly organized; that is, it is believed his brain was imperfectly organized; that he was of unsound mind; and that his derangement would increase as he grew up.

Baron Rolfe, in his charge to the jury, observed that the witnesses called for the defence, had described the prisoner as acting from uncontrollable impulse. Such evidence ought to be received with great jealousy and suspicion, because, if admitted, it might lead to the justification of every crime that might be committed.

The jury gave a verdict of guilty, but recommended the prisoner to mercy on account of his youth.

In his address to the prisoner, Baron Rolfe used language still more decided. The jury, he said, felt, that the prisoner knew too well that the act which he committed was one that would send his grandfather to his grave. An endeavour was made to show that the prisoner's understanding had not arrived at sufficient maturity to make him morally responsible; and then the more captivating defence—the attempt to establish his insanity—was made in order to induce the jury to swerve from the strict path of duty. The jury were satisfied that he had knowingly sent his aged grandfather to his account, and had therefore not hesitated to declare that he was guilty of the crime of wilful murder. The judge, in placing before the throne the recommendation to mercy, would support that recommendation; but this he should do, not on the ground of the alleged insanity of the prisoner, but on that of his youth.

PART III.

MEDICAL INTELLIGENCE.

ANATOMY.

On the Pacinian Bodies (Corpuscula of Pacini). Muller's Archiv, 1846. *Jahrbericht ueber die Fortschritte der Microscopischen Anatomie, im Jahre,* 1843.—In a former volume we directed attention to various points on the anatomical history of these bodies. (Vol. lxvii. p. 573). At present it may be proper to give a connected view of their discovery, history, and characters.

Attention was first solicited to these bodies by Philip Pacini, a physician in Pistoja, who had seen them in the year 1831, and who gave a short account of them in 1835 to the Medical-Physical Society of Florence, described them in the *Nuovo Giornale dei Letterati of Pisa,* gave a second oral communication on them from observations at the Congress at Pisa in 1839,* and published in 1840 a more enlarged account of them in a memoir entitled New Organs discovered in the Human Body, which, in September the same year, he communicated to the Academy at Paris. Subsequent communications on these bodies appeared, in 1841, in the Annali Universali of Omodei and Calderini, in which their existence was established by Guarini, while oral accounts of them were finally given in 1843 at the assembly at the town of Lucca.

About the same time as Pacini, A. G. Andral, Camus, and Lacroix, recognized these bodies in a delicate preparation of the nerves of the hand. These observations Cruveilhier mentioned in 1836, in his Descriptive Anatomy, without, however, admitting these bodies as essential parts of the nervous system.† He considers them as arising from external pressure, as they are never seen on the back of the hand, but only on the palm, and in the nerves of the members, and the sole of the foot. A. G. Andral made known his discovery first in the year 1837. He regarded them at first as ganglia of the nerves of Tact; but afterwards abandoned this view. Blandin in like manner mentioned the discovery of these three anatomists, but without considering the Pacinian bodies as essential parts of the nervous system. In October 1843, M. Lacauchie announced to the Academy of Sciences at Paris the discovery of an organ in the chylopoietic system of several mammiferous animals, which, as Pacini himself stated to the same Academy in December 1843, could be nothing else than the objects discovered‡ by him. This inquirer found these bodies in the mesentery and mesorectum of the cat. They are, according to him, elliptical, transparent bodies, with the long diameter somewhat more than one millemetre. He distinguished with the aid of the microscope a peripheral part formed of from 15 to 20 concentric layers, and a central hollow portion, which passes through the whole length of the body, terminates at one apex in a shut end, and at the other communicates by means of a serpentine canal with the nearest lymphatic vessels. In Germany, Professor Mayer of Bonn* first recognized the Pacinian bodies as distinct objects; and it may be said that German inquirers have subsequently brought the discovery of Pacini to an important and valuable scientific condition. Henle and Kolliker especially, by their Memoirs of 1844, have elucidated much the structure and anatomico-physiological relations of these bodies.

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* See this Journal, Volume liii. p. 268.

† Anatomie Descriptive, par Jean Cruveilhier, Tome iv. p. 822. Paris, 1835–1836.

‡ Observations et Propositions d'Anatomie de Chirurgie et de Medecine. These Presentée a la Faculté de Medecine de Paris, 1837.

The Pacinian bodies, which must have been often noticed by observers who studied the chyliferous vessels in the cat, were seen by Pacini first in man, and afterwards in cattle. They are seen normally without exception in adults as in the fœtus and new-born infants. Usually they are numerous in the nerves of the hand and foot, in the palm and sole. They are besides observed in the sacral plexus, in the crural nerve, in some cutaneous nerves of the upper arm and fore-arm, in the epigastric plexus and nerves issuing from it, and in the neighbouring plexuses. Their number in the whole body must be considerable; and in a single hand from 60 to 200 have been numbered. They are either detached or accumulated in small heaps. In all cases they are attached by one end by a stem variable in length to an adjoining nerve. They are for the most part distinctly visible to the naked eye; elliptical or long-oval bodies of opal lustre; and through the more translucent middle, in the long direction are observed slightly tortuous streaks. In adults their average length is from 1½ to 2 millemetres. In the fœtus they are often so small that either they cannot be seen, or they are seen by the naked eye with great difficulty. At the point of division of the median nerve, and ulnar nerve, in the fingers, and the plantar nerve in the branches going to the toes, they are largest; at the tips of the fingers they are smallest. The stems or peduncles by which these bodies are attached to the nerves, join the nerves either at a right angle, or so that they are inclined sometimes more to the central, sometimes more to the peripheral end of the nerve. Not unfrequently two bodies are attached by means of a bifurcated stem. The stems or peduncles appear to be continued backwards in a conical fashion into the *corpuscula*, by which they are easily distinguished by their transparency from the substance of the rest of the corpuscula. This prolongation amounts sometimes to one fourth part and more of the entire length of the Pacinian body.

When the *corpusculum* is examined under the microscope, it presents numerous fine dark lines, which proceed on the outside or along the periphery of the *corpusculum* by the edges, and within, run parallel to the long axis of the *corpusculum*, and are separated from each other by clear, broader intervals. At the stalk-end of the capsule, the concentric lines incline to each other, and are continued then, as is observed in large Pacinian bodies, in fine dark lines, which may be seen compressed parallel and closely to the stalk. At the free end, on the other hand, the concentric and parallel lines of the corpuscle are united on both sides; yet here there is also often observed a white line penetrating into the interior of the *corpusculum*, (*ligamentum intercapsulare,*) which appears to correspond with the prolongation of the stalk on the other side. The fine, closely compressed, parallel lines of the proper stalk become towards the nerve progressively more subtile, and in most cases vanish altogether from the eye before they sink into the nerve. In the middle of a corpuscle there is observed a longitudinal space, more or less transparent, which follows in like manner the longitudinal axis of the *corpusculum.* The dark lines lying next to it are more numerous, more closely compressed on each other, and run almost entirely straight. These concentric dark lines appeared to the observer as the outlines of so many capsules placed in layers on each other; and this view was confirmed by careful dissection. In short, when the apex of one *corpusculum* is cut off, there is observed close to the opening a small quantity of fluid, the *corpusculum* collapses, and from the interior proceeds a new *corpusculum,* more sharply defined, and smaller and more pointed than the former. This operation may be repeated several times, always with the same phenomena, except that the size of the *corpusculum* diminishes, and the oval form passes more and more to the cylindrical.

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* Ueber die Pacinischen Korperchen, Zurich, 1844.

A Pacinian body consists, therefore, of a greater or smaller number of capsules enclosing each other, which are separated from each other by a greater or less quantity of fluid, easily miscible with water. The spaces between the capsules filled with fluid are named by Pacini intercapsular spaces.

The dark parallel lines of the stalk correspond to tubes encased within each other of the sections of the membranes, which tubes are continued into the capsules, yet contain no fluid. The conical prolongation of the stalk within the *corpusculum* is formed in such a manner that the stalk of the inner capsule penetrates further into the same than the outer. The innermost tube of the stalk proceeds without expansion into the innermost capsule forming the central cavity.

The author regards the corpusculum as presiding over the preparation of the animal-magnetic fluid. The internal central cylinder he compares to the primitive nervous fibre. Henle discovered a nervous fibre in the central canal of the corpusculum.

Though the foregoing account is given from the Report of Professor Reichert, anatomists are not at all agreed either as to the nature of these bodies or their uses. All that is known is what is now stated; that by one extremity they are connected with the nervous system; and by another with the lymphatic. As to uses, it seems reasonable to think that in some manner they are concerned in the functions performed by the extremities of the nerve. But on this point there is great uncertainty.

II.—PHYSIOLOGY AND PRACTICAL APPLICATIONS.

Chloroform as a means of producing Insensibility to Pain during severe operations.—(A popular Essay on Anaisthetic Agents for procuring painless operations, particularly on the Action and Effects of Chloroform in Surgery and Midwifery, but more especially in Dental Surgery. By William Henry Mortimer, Surgeon-Dentist. London 1847. 8vo, pp. 32.)

The reign of sulphuric ether as a means of producing insensibility to pain during operations is over; and it is over a little earlier than we had some months ago predicted. The fall of sulphuric ether has, however, been accelerated by the rise of another agent for the same purpose. Chloroform —a substance in chemical constitution and properties a little different, but in physiological effects on the human body, the same, or rathe superior to sulphuric ether, is the article which has obtained, in the meantime, this distinction.

Of the chemical constitution of this substance Dr Thomson, Professor of Chemistry, Glasgow, gives the following account.

Chloroform was discovered about the same time by MM. Soubeiran* and Liebig.† Somewhat later, its properties were investigated by M. Dumas.‡

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* Ann. de Chim. et de Phys. xlviii. 151. † Ibid. xlix. 146. ‡ Ibid. lvi. 113.

It is easily obtained by distilling a mixture of alcohol and aqueous solution of *chlorite of lime*, or bleaching powder.

It is a limpid and transparent colourless liquid. Its smell and several of its other characters have some analogy with those of *hydrocarburet of chlorine.*

Its specific gravity is 1.480. The specific gravity of recent London specimens is stated at 1.493, 1.495, and never lower than 1.490. It is stated to be made in London so strong as to be 1.5054 at 48° of Fahrenheit. It boils when heated to the temperature of 141½.° The specific gravity of its vapour is 4.199.

It is not inflammable. But if a glass rod moistened with it be put into the flame of burning alcohol we observe a yellow fuliginous flame. Hydrocarburet of chlorine, in the same circumstances, burns with a large luminous flame, the lower edge of which is coloured green.

If vapour of chloroform be passed over red-hot iron or copper it is entirely decomposed. We obtain a metallic chloride covered with charcoal; but no inflammable gas, if the experiments of Liebig be accurate. If its vapours be passed through a glass tube raised to a dull red heat we obtain a small quantity of gas, a portion of which is absorbed by water, and another is combustible and burns with a green-coloured flame. The inner surface of the tube becomes black and is covered with a number of white filamentous crystals, which, (judging from their smell) have a great resemblance to Faraday's solid chloride of carbon.

It is decomposed by lime at a dull-red heat, and no trace of inflammable gas disengaged can be observed. At a higher temperature carbonic oxide gas is obtained, occasioned by the action of the charcoal deposited on the carbonate of lime produced.

Chloroform is not decomposed by potassium. It may be distilled off that metal without occasioning the smallest alteration. At the commencement of the process, indeed, some bubbles of hydrogen gas may be observed, which increase when the liquid is raised to the boiling temperature. When potassium is heated in the vapour of chloroform it burns with an explosion, chloride of potassium being formed and charcoal deposited.

When long boiled with the alkaline hydrates chloroform is converted into chloride and formate of the alkali used.

Alcohol and ether dissolve it readily; but water throws it down from these solutions. It dissolves sulphur, phosphorus, and iodine, without undergoing any alteration in its characters.

When chloral is mixed with milk of lime, potash, or barytes water and distilled, chloroform is obtained. A very dense, limpid, and transparent liquor comes over. It must be repeatedly agitated with pure water, and, after having drawn off the greatest part of the water with a sucker, a quantity of sulphuric acid, about six or eight times the volume of the residual liquid, is added. The whole is violently shaken, and the chloroform, which swims on the surface, is drawn off. It is finally distilled off barytes in a dry apparatus. In this way chloroform free from water may be obtained.

But the easiest method of obtaining it is by means of bleaching powder, as mentioned at the beginning of this section. Mix together 1 pound of chlorite of lime, 3 lbs. of water, and 2 or 3 ounces of spirit of wine, and distil. As the liquid froths much during the distillation, the process must be conducted in a large retort.

Acetone distilled in the same way with chlorite of lime yields chloroform in still greater abundance.

Chloroform has been subjected to analysis by Soubeiran, Liebig, and Dumas. Dr Thomson considers the result obtained by this last chemist as nearest the truth. He had the advantage of the labours of the two former experimenters to guide his processes.

The mean of three analyses agreeing very well with each other, give the constituents of chloroform as follows:—

Carbon	10·11	or 2 atoms =	1·5	or per cent.	9·92
Hydrogen	0·91	or 1 atom =	0·125	— —	0·83
Chlorine	88·98	or 3 atoms =	13·5	— —	89·25
	100·00		15·125		100·00

If we consider its vapour as composed of

2 volumes carbon . .	= 0·8333
1 volume hydrogen gas . . .	= 0·0694
3 volumes chlorine gas .	= 7·5
	2)8·3997
	4·1998

condensed into 2 volumes, its specific gravity would be 4·1998. Now we have stated above that Dumas found its specific gravity to be 4·199. Thus there can be no doubt whatever about the constitution of chloroform. It is a compound of 1 atom bicarburet of hydrogen with 3 atoms chlorine.

It appears from the weekly Reports (*Comptes Rendus*) of the sittings of the Royal Academy in March, that M. Flourens had thought of trying the physiological effects of chloroform on animals. He found it to answer completely in producing a state of temporary insensibility. He does not appear, however, to have carried his inquiries further.

Dr Simpson, Professor of Midwifery in the University of Edinburgh, several months ago tried the effects of chloroform in abating the severity of pain during parturition; and he found, after various trials, that it was equally effectual in inducing the state of insensibility to pain, and could be administered with greater facility, and under circumstances in which sulphuric ether inhalation is either administered with difficulty, or is altogether impracticable. The following summary of the advantage of administering this substance, and of its effects, is given by Dr Simpson.

" As an inhaled anæsthetic agent, it possesses all the advantages of sulphuric ether, without its principal disadvantages.

" A greatly less quantity of chloroform than of ether is requisite to produce the anæsthetic effect—usually from a hundred to a hundred and twenty drops of chloroform being sufficient, and with some patients much less. I have seen a strong person rendered completely insensible by seven inspirations of thirty drops only of the liquid.

" Its action is much more rapid and complete, and generally much more persistent. Dr S. almost always saw from ten to twenty inspirations suffice—sometimes fewer. Hence the time of the surgeon is saved, and that preliminary stage of excitement which pertains to all narcotizing agents, being curtailed, or, indeed, practically abolished, the patient has not the same degree of tendency to exhilaration and talking.

" Most of those who know, from previous experience, the sensations produced by ether inhalation, and who have subsequently breathed the chloroform, have strongly declared the inhalation and influence of chloroform to be far more agreeable and pleasant than those of ether.

" Its perfume is not unpleasant, but the reverse, and the odour of it does not remain, for any length of time, attached to the clothes of the attendant, or exhaling, in a disagreeable form, from the lungs of the patient, as so generally happens with sulphuric ether.

"No special kind of inhaler or instrument is necessary for its exhibition. A little of the liquid diffused upon the interior of a hollow-shaped sponge, or a pocket-hankerchief, or a piece of linen or paper, and held over the mouth and nostrils, so as to be fully inhaled, generally suffices in about a minute or two, to produce the desired effect.

"I have had an opportunity of using chloroform with perfect success in several surgical operations (removal of tumours, of necrosed bone, partial amputation of the great toe), and in tooth-drawing, opening abscesses, &c. I have also employed it in obstetric practice with complete success.

"No sickness, vomiting, headach, salivation, uneasiness of chest, in any of the cases. Once or twice a tickling cough took place in the first breathings.

"The employment of chloroform evidently offers great and decided advantages in rapidity, facility, and efficiency over the employment of ether. When used for surgical purposes, I would advise it to be given upon a handkerchief, gathered up into a cuplike form in the hand of the exhibitor, and the open end of the cup placed over the nose and mouth of the patient. For the first inspiration or two it should be held at the distance of an inch or so from the face, and then more and more closely applied to it. To insure a full and perfect anæsthetic effect—more especially when the operation is to be severe—a tea-spoonful of the chloroform should at once be placed upon the hollow of the hankerchief, and immediately held to the face of the patient. Generally, a snoring sleep very speedily supervenes; and when it does, it is a perfect test of the superinduction of complete insensibility. But many patients are quite anæsthetic without this symptom."

It is manifest that, if an anæsthetic agent is at all or on any occasion to be employed, then chloroform possesses many advantages over sulphuric ether. One advantage is at first sight obvious. It requires no inhaling apparatus, and this is no trifling recommendation.

Amputations of various kinds, excision of tumours, operations for the extraction of dead *sequestra* in nekrosis, generally a painful and tedious process, have all been performed on patients who have been previously put in a state of insensibility, by inhalation of the vapour of chloroform. In one instance in which a female had suffered on the Glasgow Railway, an injury of both legs so extensive as to require amputation, the patient was first put in a state of inconsciousness by the vapour, and then the operation was performed with such total absence of sensation on her part, that she was not aware of the fact till some time afterwards.

The employment of chloroform vapour as an anæsthetic agent has been in like manner resorted to in the London Hospitals. During the last two weeks of November, namely, between Monday the 15th, and Monday the 29th, chloroform was employed in various metropolitan hospitals. At King's College Hospital, by Mr Fergusson, in one case an eye affected with melanosis was extirpated from a patient under its influence on Saturday the 20th November; in another case, the operation for *fistula lacrymalis* was performed; and in a third iodine solution was injected into the *tunica vaginalis* for the cure of hydrocele; all apparently with little or no indications of suffering on the part of the patient.

The following week operations were performed on persons under its influence at University College Hospital by the late Mr Liston; at St Bartholomew's Hospital by Mr Lawrence; at St George's by Mr Tatum, and various other places. Among these operations are included lithotomy, excision of the mamma, excision of tumours, and others usually accompanied with considerable suffering.

The following letter from Mr Skey to Mr Mortimer gives the result of observations at St Bartholomew's Hospital.

Ether "has now been employed in the operating theatre of St Bartholomew's Hospital, and frequently in your presence, and also in the surgery of that institution, to the amount of nearly two thousand cases; and I cannot say that I am cognisant of one example of injurious effects produced. I do not say that the results have been uniformly successful, because, from some unexplained cause or other, a few examples have occurred in which the ether has been apparently respired, either without being absorbed by the lungs, or without these few patients being amenable to its influence. The aggregate of these exceptions, however, would not exceed half a dozen among the larger operations.

"However apparently innocuous this agent may be, it is impossible to witness the struggling, the congestion of the vessels of the face and head, and the marked indication of what we should at other times term apoplectic symptoms, with indifference, although these symptoms, strongly as they are often manifested under the protracted exhibition of ether, quickly subside on the removal of the cause. I confess that I should hesitate in the employment of the vapour of sulphuric ether, in cases in which I was previously admonished of a determination of blood to the head, or the existence of a former tendency to apoplexy.

The chloroform exhibits these objections in a far less formidable degree, though not totally without them. Its advantages appear to me, first, That it may be inhaled with less repugnance by the patient; secondly, That its influence is more quickly obtained, and longer retained; thirdly, That it produces less convulsion and suffering of all kind; on the contrary, that its effects are agreeable, and are frequently relinquished by the patient only by a positive effort of the administor. Fourthly, That though highly volatile it does not attach itself to the clothes of the attendants, a very positive objection to the use of ether by medical men congregating about an operating-table. In my opinion the discovery of these agents, by which human suffering is so largely abridged, is among the very highest improvements of modern science.

"The first case in which I employed the chloroform was in that of a girl of about seventeen years of age. About forty drops were poured on a sponge and held to her mouth. In two minutes she was under its influence. A large molar tooth was extracted, in a state of unconsciousness on the part of the girl, without struggle or contortion of face. She did not utter a sound either before, during, or after the operation. She lay in a placid sleep for six minutes after its removal, and then awoke in perfect health.

"A boy aged twelve underwent for a second time the operation for the stone. During the preparation he cried violently and incessantly, to the great annoyance of the attendants around. The chloroform was applied as before described. In sixty seconds he lay perfectly motionless; underwent the operation without the smallest suffering or consciousness, and awoke in two minutes after it was completed.

"Another boy, aged ten, who had previously undergone a painful operation, showed the greatest disinclination to submit to the requisite preparation, and struggled violently. The chloroform was administered to him by means of the sponge, and in one minute he was unconscious. The operation was necessarily a protracted one, and it became necessary on his awaking to repeat the inhalation, which from time to time was continued at short periods. The agent was here eminently successful, for the operation was completed without suffering of any kind. It appears to me that we labour under one small difficulty in the administration of these anæsthetic agents, viz. our ignorance of the precise moment at which sensation is suspended. Now, it is in this very particular that I regard the employment of chloroform as a great improvement on the sulphuric ether, because no injury is sustained to the patient, even though the quantity of the vapour administered be greater than is required to meet the necessities of his case. Its anæsthetic influence is more positive, and the constitutional effects less lasting.

The quantities requiring to be converted into vapour and inhaled vary in different cases. But in general they are not great. Mr Mortimer gives the following table of quantities, supposing the chloroform to be pure and of average strength.

Age.	Quantity put on the Sponge.	Time required to produce insensibility.	Duration of the Effect.	Remarks.
5 to 10	15 drops	20 sec.	12 min.	In these calculations it is assumed that induction is not continued after insensibility is complete.
11 to 14	20 „	20 to 30 „	10 about	
15 to 20	30 to 40 „	40 about	7 to 12	
Adults, male	1 to 3 drams.	1 to 3 min.	8 about	
—— female	1 to 2 „	1 to 2 „	7 to 10	

Mr Mortimer makes some observations on the uses of this agent to the dentist, whose operations often cause pain and suffering much more severe than other operations more important :—

"Simplicity in the administration of Chloroform or any other anæsthetic agent, is one of the great desiderata to be obtained. The plan I adopt will, I believe, answer for all purposes, certainly for all cases of dental surgery, where inhalation does not require to be prolonged after superinduction is once complete. I use a small hollow oval sponge covered outside with oilskin; or, but for its smell, a similarly formed piece of the sponio piline epithem. The impermeable outside covering preventing any waste by the escape of the chloroform on the hands of the exhibitor, and making it more prompt and effective in its action. I generally allow the nostrils to remain free for the first two or three inspirations, as being more agreeable to the patient, and not at all interfering with its speedy effect. Lint and cotton are, I think, to be objected to, except in experienced hands, as risking the introduction of the fluid into the mouth; and generally vesicating the lips by its contact. All inhaler apparatuses are quite unnecessary and answer no good purpose whatever, but rather the reverse, as causing unnecessary alarm to the patient, by their formidable appearance.

"Very few rules and directions are necessary respecting the mode of its administration. 1st, As little excitement as possible should precede the operation, and no unnecessary alarm should be created in the mind of the patient. 2d, From the moment induction commences, until the patient is perfectly recovered from its effects, he should be left perfectly quiet; all talking, questioning, and excitement of every kind should be carefully avoided; and, 3d, Care should be taken not to administer a larger dose of the vapour than the nature of the operation requires. If these indications are strictly fulfilled, I may safely affirm, what I have found invariably to be the case, that no unfavourable symptoms ever follow."

Lastly, it appears that even in brute animals chloroform vapour exerts a narcotic and stupifying influence so great, that it may be administered in situations in which the administration of ether would be impracticable. The following example of its administration to a quadruped under circumstances of great danger illustrates this part of the history of chloroform with considerable force :—

On Sunday evening, 5th December, as Mr Garner, veterinary surgeon of St Ives, was returning through Cambridge with his horse and gig, from a visit to his brother at Hawiston, he had occasion to alter the harness, and for this purpose he stopped at the Little Rose; but he had scarcely got out of the gig when the horse began to kick in a most alarming manner, to the imminent danger of a lady who remained in the gig, as well as that of Mr Garner himself, who was holding the animal by the head. Many persons were collected; but such was the fury of the animal, that no one could render any assistance. At last the horse threw himself down, Mr Garner still holding by the head; and when in this position, Mr Cox chemist, next door, apprehensive of the danger that might ensue, poured about one drachm of chloroform on his handkerchief, and applied it to the animal's mouth and nostrils. The ferocious quadruped inhaled the vapour, and in one minute became quite insensible and to all appearance dead. Meanwhile, the gig and harness were removed; and in about five minutes the effect of the chloroform passed off, the horse got up, shook himself, and walked quietly into the stable.

On the propriety or necessity of employing chloroform vapour to produce insensibility on all occasions previous to operations, it is unnecessary to give any opinion. Neither would it be seasonable here to consider the question of secondary effects. It is impossible to doubt that the vapour causes temporary stupefaction in all respects similar to that of intoxication, unless that its effects pass off much more rapidly. Objections have been urged against the use of sulphuric ether, on the ground that the presence of an agent containing so large a portion of carburetted hydrogen in the blood, produces in that liquid permanently bad effects. It is not easy hitherto to say, whether the same objection be in any mode applicable to chloroform vapour; and as its effects are much more transitory, and its composition a little different, it is possible that it may not. All that can at present be said regarding chloroform is, that under circumstances, and in situations in which it is requisite to produce temporary insensibility and inconsciousness, it seems to answer the purpose perfectly well, more accurately certainly than sulphuric ether. The question as to the propriety or the necessity of administering either in all cases where pain may be apprehended, is totally different, and must be determined on different grounds.

III.—MEDICAL PATHOLOGY.

Cholera at Moscow.—By the last accounts received at St Petersburg from Moscow, cholera at the latter city has been declining since the 3d of November 1847. On the 5th November, among 94 cases, 49 persons died, and 39 recovered, the other 6 being under treatment. On the 9th November, 77 cases were reported, among which 22 died and 32 recovered. The disease appeared in Moscow first on 30th of September 1847; and since that time the number of cases has been 2011. 930 persons died; 422 recovered; and 639 are still under treatment.* It is supposed that the majority of the latter will recover.

From Moscow the disease has been extending to other localities in the same government.

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* Attacked,	2011
Died,	930
Recovered,	422
Under treatment,	639
	1991
Unaccounted for,	20

At Kiev, according to the most recent information, the disease is on the increase. At Kasan it is rapidly declining. In other parts of the empire it presents a less destructive character.

In the passage of this disease from the government of Astrakan to that of Saratov, the following facts have been observed. 1. In its progress towards Saratov, the epidemic has constantly proceeded against the direction of the wind. 2. As in 1830, it left in 1847 the colony of Sarepta untouched, though the communications between the last spot and the environs have not been interrupted. This is ascribed to the position of Sarepta; for it possesses nothing distinguishing it from other localities in the Wolga; but the exemption ought rather to be explained by the circumstance that in Sarepta there are few of those ranks called inferior, who are generally most severely attacked by cholera. The inhabitants of Sarepta are industrious, in sufficiently easy circumstances, and are models of cleanliness and sobriety. The town is a colony or settlement of Moravian brethren, founded in 1766, in a country filled with salt mills. 3. In advancing from Kamuchen to Saratov, cholera spared foreign settlements on the route. Afterwards it returned to Kamuchin; but at a time when its intensity at Saratov had abated.

In these villages of settlers, cholera broke out immediately after the arrival of travellers coming from Saratov, and who felt themselves already unwell at their arrival, or who were attacked with cholera very soon after. The period of incubation did not in these cases exceed four days. From this it is inferred that the disease is communicated by the persons of those labouring under it.

It was believed during the presence of the disease, that it was possible to observe at Saratov, that the inhabitants of localities actually attacked were less exposed to the disorder, in their relations with patients, than persons who arrived from a country yet untouched by it. The epidemic character of the disease might be observed in those who suffered during the prevalence of the disease, a sense of oppression at the pit of the stomach, general uneasiness and sleeplessness, though the rest of the system no derangement.

Cholera broke out on the 29th September 1847, in the government of Kaluga (district of Lichwin); and on the 21st October, in the town of Kaluga, where, in the first week, 7 cases took place, among which 5 were fatal. On the 1st November, the disease appeared in the town of Kechitzka, government of Minsk, causing at first 4 cases, 3 being fatal.

Paris, 30th November 1847.

IV.—SURGERY.

Instance of Rupture of the Urinary Bladder terminating in recovery of the patient. (*Contributions to the Pathology and Practice of Surgery.* By James Syme, F. R. S. E., &c. Edinburgh, 1848, p. 332.) In this Journal and various other periodical publications are recorded sundry cases of rupture of the nrinary bladder. In the majority of, if not all of the cases, the rupture took place in consequence of a fall when the bladder was distended, and in some instances by the individual alighting on the belly or striking the hypogastric ragion against some prominent body. In the majority, probably all the instances, also the rupture terminating fatally, by the effusion of urine within the abdomen, and the consequent establishment of *peritonitis* The following case, which is published by Mr Syme, is probably the only instance in which recovery has taken pace after so severe an accident.

"On the evening of the 5th of July, I was requested, by Messrs Joseph and Benjamin Bell, to see with them a young gentleman who seemed to have had his bladder ruptured. Circumstances prevented me from meeting these gentlemen until midnight, when I learned from them that the patient, a stout youth, seventeen years of age, after dining with his family, had gone out to take a walk, in the course of which he had encountered a low paling, about two feet high, and attempted carelessly to leap over it, but, instead of doing so, had fallen forwards, so as to strike the lower part of his belly with great force on the points of two upright spars of wood. He immediately complained of intense pain, and of a feeling as if his bowels had protruded; his brother, who had accompanied him, at the same time remarking that his clothes were distended over the belly. With the aid of support on each side, he then accomplished a few steps, so as to reach a carriage that had been brought as near as possible to the place where he was lying. A catheter had been introduced, and, after drawing off four ounces of bloody urine, allowed to remain in the bladder. We found all the usual signs of ruptured bladder; there being great pain and distension of the belly, with a sunk, anxious look. Twenty leeches and hot fomentations were applied. The catheter was taken out, and an opiate prescribed.

"On the following day the abdominal pains and swelling were increased, there being dulness on percussion below the umbilicus, and more than ordinary resonance above it. The catheter was introduced with the effect of withdrawing a few ounces of bloody urine. In the evening, leeches were again applied, and an opiate prescribed.

"Next day, the 6th, he was found to have passed a restless night. There was some confusion of ideas, and considerable impatience for a change of posture, &c. The abdominal swelling had increased, and there was some œdema of the posterior parts from the chest down to the thighs. The catheter was introduced twice, and each time drew off a quantity of bloody urine, similar to that which had been obtained on the former occasion.

"On the 8th he was in much the same state, and not sinking, as we had fully expected to find him; but the whole trunk was fearfully swollen, and his respiration was performed as if only a small portion of the lungs had room to act. The œdematous effusion had greatly increased at the lateral and lower parts, while the tympanitic condition was still more manifest anteriorly and superiorly. Below the umbilicus there was not only complete dulness on percussion, but obscure fluctuation, which, after careful deliberation, induced us to think that an incision in the linea alba, a little way above the pubis, might be of use. After cutting through a thick mass of condensed texture at this part, I saw a stream of clear fluid begin to trickle out, and, wishing to see the effect of what had been done before proceeding further, we ordered a large soft sponge squeezed out of warm water to be applied so long as the fluid should continue to escape. In the evening it was ascertained that a very large quantity of urine had passed from the wound, and the abdominal swelling was considerably reduced, while in all other respects the patient appeared to have experienced relief. The catheter had been introduced twice during the day without obtaining a drop of water, although nearly a tumblerful was taken off by it in the morning before the incision.

"On the 9th the pulse had fallen to 100. The abdomen was greatly collapsed, and every thing seemed favourable, and he continued in this satisfactory state until the following evening, when it was observed that the urine ceased to come freely away, and he became restless, with a return in some degree of his former unpleasant symptoms.

"On the 11th there was considerable swelling of the lower part of the belly, with quick pulse and foul tongue. The wound looked dry and white, as in a patient after lithotomy who is suffering from inflammation at the neck of the bladder. We entertained serious apprehensions of the sequel, but thought it right to enlarge the aperture lest there should be any obstruction to the flow of urine. I effected this by means of a bistoury, so freely, that my finger could be introduced down to the muscles, which were found separated from each other in the mesial plane to the extent of an inch and half, so as to form a narrow slit through which the water passed.

"On the 12th the belly was collapsed. The pulse had fallen to 80, and the patient was quite easy.

"No particular change occurred until the 19th, when a considerable quantity of sloughy cellular substance was extracted from the wound; and on the 21st, in consequence of there being a rather copious discharge of matter from the cavity lying between the integuments and muscles of the abdomen, a free counter opening was made on each side, as low in the flank as the point of a catheter could be pushed. A great improvement speedily followed the establishment of these drains so far as the superficial parts were concerned, although there was still a copious issue of thick matter from the aperture between the muscles.

"On the 26th, seeing a slough at the orifice, I seized it with dressing forceps, and gradually pulled out a bag bearing no small resemblance to the bladder, which was found to consist of dead cellular substance, lined with a white deposit from the urine. I then passed my finger down into the bladder and felt a rent more than an inch long in the anterior part of the fundus without, or on the pubal side of the reflection of the peritoneum.

"On the 5th of August the patient very unexpectedly passed seven ounces of water by the urethra, though the catheter had been frequently introduced before without obtaining so much as a tea-spoonful, ever since the opening above the pubis was made. In the course of a fortnight the patient was in every respect perfectly well, and quickly regained his strength without the slightest trace of inconvenience from the injury.

"As there is not upon record, so far as I know, any well authenticated instance of recovery from rupture of the urinary bladder by violence, this case may be regarded as of some interest, and also, perhaps, prove of practical use on such occasions, by suggesting the possibility of affording relief through local treatment. Mr Benjamin Bell informs me, that while he was a resident pupil in Bartholomew's hospital, it was found, on examining the body of a child, that in addition to other injuries of a mortal kind caused by the wheel of a carriage, the bladder was ruptured precisely in the same situation as in the patient whose case has just been related. If the rupture takes place above or within the reflection of the peritoneum, there cannot be the slightest chance of escape. But if the rent is at the anterior part, so as to discharge the contents of the bladder by a sudden gush into the cellular substance, and condense it in such a way that merely the portion in contact with the urine is deprived of life, it appears that the patient may be saved by timely incisions."

V.—OBSTETRICS.

Table VI.—*Showing the age at which the last pregnancy terminated and that at which menstruation finally ceased, in thirty-eight married women; the temperament or the habit of body; and the state of health accompanying the last menstrual change, in each individual respectively.* (This is the table referred to in p. 170.)

No.	Age at termination of last pregnancy.	Age at the final cessation of the menses.	Temperament or habit of body.	State of health at the last menstrual change.
1	33	24	Nervous-sanguine.	Favourable.
2	30	54	Strumous.	Unfavourable.
3	30	40	Bilious.	Unfavourable.
4	32	52½	Sang.-lymphatic.	Favourable.
5	35	50	Sanguine.	Unfavourable.
6	36½	37	Nervous-sanguine.	Favourable.
7	37	44	Lymph.-sanguine.	Favourable.
8	38	38	Sanguine-bilious.	Favourable.
9	39	50	Bilious.	Unfavourable.
10	40	46	Sanguine.	Favourable.
11	40	45	Strumous.	Unfavourable.
12	40	49	Bilious.	Unfavourable.
13	40	41	Bilious-sanguine.	Unfavourable.
14	41	45	Sanguine.	Unfavourable.
15	41	45	Lymphatic.	Favourable.
16	42	48	Sanguine.	Favourable.
17	42	46	Bilious.	Unfavourable.
18	42	51	Bilious.	Favourable.
19	42	66	Lymph.-sanguine.	Unfavourable.
20	43	48	Lymphatic.	Favourable.
21	43	51	Lymph.-sanguine.	Unfavourable.
22	43	48½	Lymph.-sanguine.	Unfavourable.
23	44	49	Bilious.	Favourable.
24	44	52¼	Bilious.	Favourable.
25	45	50	Sanguine.	Favourable.
26	45	51	Bilious.	Unfavourable.
27	45	48	Bilious.	Favourable.
28	45	48	Bilious.	Favourable.
29	45	47	Bilious.	Unfavourable.
30	46	47	Sanguine-bilious.	Favourable.
31	46	50	Sanguine-bilious.	Favourable.
32	46½	47	Bilious-sanguine.	Unfavourable.
33	47	52	Lymphatic.	Unfavourable.
34	47	47	Lymphatic.	Favourable.
35	47	48	Strumous.	Favourable.
36	47	50	Sanguine.	Favourable.
37	48	49	Sanguine.	Favourable.
38	49	52	Sanguine.	Favourable.

VI.—HYGIENICS, MEDICAL POLICY, AND MEDICAL STATISTICS.

Effects of the Disinfecting Fluids of Sir William Burnet *and* Mr Leydoyen.—We have been favoured with the following statement by Dr Stratton, the gentleman appointed by government to test the merits of the *disinfecting Fluids*, about which so much has been recently said in the Quebec papers.

It appears that Sir William Burnett's has been pronounced the most efficacious remedy, there being seven voices in favour of Sir William's against three for Mr Ledoyen's:—

Quebec Marine and Emigrant Hospital, Saturday, October 9, 1847.

It having been proposed by Dr Painchaud, Senior Physician of the Hospital, that the following method should be followed for the purpose of making a comparative trial of the Disinfecting Fluids of Sir William Burnett and of Mr Ledoyen, viz:—

1. That each operator should take a basin one-third filled with fæces, taken from the same pail.

2. The names of the operators to be marked, one on the bottom of each basin.

3. An equal quantity of disinfecting fluid, and also of the same strength, to be put into each basin.

4. Both basins to be stirred for the same length of time.

5. The gentlemen of the hospital to be requested to point out the one which gives out the least smell.

6. And for the removal of any partiality, those giving their opinion not to know until the opinion is given, to whom such or such a basin belongs.

7. A written deposition to be made out and signed, signed by those giving their opinion.

And it having further been proposed by Dr Stratton, R. N., that as one of the fluids changed feculent matter to a whitish colour, it would better secure an impartial opinion if the judges would blindfold themselves. This also was agreed to, and in the presence of witnesses, Dr Stratton and Mr Ledoyen, each put in equal quantities of the fluids, and in fifteen minutes after the basins were examined by the umpires; the basins were placed, one on the right and the other on the left, and some distance apart; if the umpire thought that the basin on the right had least smell, he called out " right," and so on. All the medical gentlemen in the hospital at the time, except Dr Stratton, gave their opinion. Eleven voted, and as follows:—

A. That the smell in both basins was equal; one vote, Dr Jas. Douglas.

B That the right basin had least smell; three votes, namely, Messrs Martin, Lemieux, and Whelan.

C. That the left basin had least smell; seven votes, namely, Dr Kimlin, Messrs Blanchet, Painchaud, Jun., Prendergast, Connolly, Reed, and Colonel Calvert.

[This last gentleman, Colonel Calvert, afterwards stated that his vote had been erroneously taken; he said, he meant that the left basin smelt the most; this however was done after Colonel Calvert had had an opportunity of perceiving the different colours of the contents of the basins, as he was not then blindfolded.]

On looking then at the bottom of the basins, the left one was marked Sir William Burnett's, and the right one, Mr Ledoyen's.

Mr Martin, Hospital Apothecary, was requested to act as secretary.

We, the undersigned, certify that the foregoing statement is correct.

(Signed) Jos. Painchaud, M. D., Senior Physician, Quebec Marine Hospital.
J. Douglas, M. D., Physician, Quebec Marine Hospital.
William Kimlin, M. D.
J. Blanchet, M.R.C.S., London.
J. Martin, Apothecary to the Hospital.
P. Prendergast.
P. Connolly.
J. Reed.
W. Whelan.

Temperature, &c. of Penetanguishene, Canada, in 1846 *and* 1847. By THOMAS STRATON, M.D., Edin., Surgeon in the Royal Navy. In a Letter to the Editor of the Edinburgh Medical and Surgical Journal.

In the Edinburgh Medical and Surgical Journal for January and July 1847, I gave a meteorological account of this part of Canada for several years, and I now beg to offer some further observations on the same subject.

	In the 24 hours External Register Thermometer.				External Thermometer in the shade at 9 A.M.		Number of days.						
	Lowest in the previous night.		Highest in the shade in the day.				Wholly fair.	Partly rainy.	Wholly snowy.	Partly snowy.	Hail.	Sleet.	Thunder.
1846	Min.	Max.	Min.	Max.	Min.	Max.							
Jany.	—18°	33°	9°	45°	— 7°	35°	15	3	1	10	1	3	...
Feby.	—23	32	3	46	— 6	36	10	3	...	17	..	2	...
March	— 8	40	34	60	11	47	16	10	...	7	...	...	...
April	20	58	40	82	27	64	15	11	..	3	1	...	...
May	31	60	59	95	32	74	21	10	...	...	...	...	4
June	...	...	60	90	56	93	24	6	...	...	...	...	2
July	...	...	70	94	60	88	22	8	...	...	...	...	6
August	...	...	71	96	64	86	23	8	...	...	...	...	3
Sept.	...	...	...	...	...	...	..	..	...	..	.		...
Oct.	20	55	33	81	28	80	12	16	..	2	2	...	3
Nov.	15	44	25	70	22	53	10	14	..	3	3	2	1
Dec.	...	30	20	55	5	37	15	8	...	15	2	1	...
1847.													
Jany.	—17	28	13	39	— 3	37	7	3	2	23	...	...	1
Feby.	—18	33	17	54	1	39	10	2	1	16	1	...	...
March	—11	31	17	57	8	43	17	7	...	10	1	...	...
April	1	39	37	57	28	51	14	9	...	7	2	...	1
May	22	56	40	89	37	74	20	11	...	...	...	...	1

By day is meant the 24 hours. There were only two days of Indian summer, the 6th and 7th of October. The navigation was closed on the 1st December and opened on the 1st of May.

PENETANGUISHENE, LAKE HURON,
1*st June* 1847.

Metropolitan Sanitary Commission.—The Report of the Metropolitan Sanatory Commissioners, &c., contains several points deserving attention.

1. In the first place, it is at length admitted, after ample investigation of evidence, that cholera neither arises nor is propagated as a contagious disease, and that whatever be its causes, it is not transmitted from person to person. This is a very important step in hygienic proceedings, and involves the adoption of a new and quite different set of sanitary regulations. It affords us sincere pleasure to observe that this doctrine of the non-contagiousness of cholera, which we showed in 1832 is the correct one, and the only one consistent with facts, has at length been recognized in this manner, and that it is recommended to be the basis of any practical measures, should that disease again visit the British dominions.

Assuredly no greater error could be committed, in every point of view, than that which in 1832 received the sanction of all the public boards in this country. This doctrine misled the public entirely as to the true causes of the distemper; and authorized them, while enforcing all sorts of grievous and impracticable measures of segregation, seclusion, and non-intercourse, at a most serious waste of money in every department, to overlook and neglect the true sources of the epidemic, and accordingly, to be quite inactive in the adoption of measures most likely to be efficient.

We are not at present to resume this question. But we may observe that it is at length admitted by the reporters that the disease arises in situations abounding in humidity, and where there are sources of vegetable and animal decomposition. Water-courses, banks of rivers, low flat moist grounds, ditches and marshes, drains, when foul and obstructed, have all been observed to be the favourite localities for the rise and propagation of cholera. In a very large proportion of cases, the first places in which the disease appeared in 1832 was on the banks of rivers. In other instances it appeared near ditches and marshy grounds. It seems also occasionally to be a disease of mere atmospheric origin, at least propagated by a particular state of the atmosphere. But that state is always connected with great humidity.

This admission of the non-contagiousness of cholera, and the doctrine of its dependence upon local causes, will not, however, justify the neglect of those precautions as to crowding many persons together, which some might think may be done with impunity. Next to removing sources of nuisance, as clearing drains, ditches, and water-courses, embanking, and, if necessary, removing inhabitants from a locality incurably unhealthy, must be placed the dispersion and separation of the population, the preventing many from being crowded together either by day or night, and, above all, the adopting of measures to ensure cleanliness, dryness of the soil, and constant supplies of fresh air.

2. Next to the recognition of the doctrine that cholera may arise and be propagated without contagion, may be placed another allied and analogous doctrine, in which it would have been desirable that a positive expression of opinion had been made. This is, that there is no reason to believe that dysentery and diarrhœa are propagated by contagion. Many physicians, it is well kown, maintain that dysentery is a contagious distemper. It is difficult to conceive how this opinion can be maintained. It is certain that it prevails in exactly the same situations and under the same physical and telluric conditions as ague and remittent fever. It is a distemper of miasmatic or malarial and telluric origin, often manifestly connected with sources of local filth and decomposition. The experience of the summer of 1846 in this country, when diarrhœa and dysentery prevailed in some towns and districts to a very great extent, is sufficient to show that these disorders may arise and be propagated independent of contagion. Of the operation of this principle, there was during that summer no evidence; and it was accordingly rarely mentioned. The real causes, in truth, were sufficiently obvious, and excluded all imaginary ones.

If, therefore, cholera again visit this country, it is to be hoped that the confusion, disorder, inertness and contradictory proceedings of 1832, will not take place. The epidemic itself is bad enough. But when it is aggravated by the panic and confusion which result from the authorities and rash medical partizans, declaring it to be contagious, it is difficult to conceive all the evils produced.

3. In the third place,—with regard to typhus, the commission announce their opinions that the disorder is equally capable of being prevented by proper precautions as to cleanliness and ventilation. As to the contagious character of the disease, they give no opinion; and probably it is not easy to form a decided judgment. Is is certain that most experienced phy-

sicians in London, have seen reason to think that typhus is, in that city, not a very contagious distemper. Such was the opinion of Baillie, after seeing the disorder in St George's Hospital. It is also a common practice in the hospitals of that city to place fever patients in the same wards with ordinary medical and even surgical cases; a practice which could not be continued were the disease very contagious.

It is to be further remarked, that typhus is often observed to spring up in certain situations, and attack individuals without any proof of the operation of contagion. Exposure to cold, getting wetted, and various other casualties, are, in such circumstances, common precursors of the disorder; but no evidence can be obtained, that the individual suffering has been in communication with a person previously ill.

There are, however, prevalent in London two diseases, to which the name of typhus is applied. One is the ordinary typhus of populous and crowded towns, most prevalent, generally, in winter, and cold moist weather, probably contagious, though in no great degree, at least liable to spread by contagion, among persons crowded together where the air is impure, and imperfectly changed, and where little attention to cleanliness is given. This appears to be not very common in London, unless in densely peopled and much crowded districts.

The other distemper to which the name of typhus is given is intestinal typhus, or what has since been denominated typhoid fever, and is not typhus at all. It is indeed a remittent-continuous fever, which towards the close of the disease assumes typhoid symptoms. It affects principally the intestinal canal and the intestinal follicles or glands of Peyer. This disease is evidently not contagious either in origin or in mode of propagation. Its presence depends upon the operation of local causes, most likely miasmatic or telluric, or these combined with atmospheric conditions. It is this disease which prevails most in London, and is usually seen in the London Hopitals. It is a common form of fever in many German towns, in Paris, and in similar places. But in none of the situations in which it has been observed has it been known to be propagated by contagion. This may explain the fact, that in London most observers of this disease ascribe its prevalence to the vicinity of drains, cess-pools, and similar receptacles of moist filth.

4. In the fourth place, it appears that the back streets, lanes, courts, and alleys of the metropolis, are very much in the same state in which they were in the year 1832. This is indeed a melancholy fact; and what is stated of London may, we believe, be said of most other large, populous, and crowded towns. The truth is, that there is in these places a mighty error in the original plan and construction of all these lanes, courts, alleys, and closes in large towns. Nothing can be more favourable to the accumulation of filth than building houses so as to form courts and closes; and ventilation is in a manner rendered impracticable. If houses be arranged in the form of a court, openings ought to be left at two opposite sides; and then it is no longer a court. Lanes and alleys are little better, for by the limited space allowed between the opposing rows of houses light and ventilation are equally obstructed; and accumulations of filth of all kinds are favoured.

Another evil not contemplated in the original construction of houses in London and other large towns is the following. As the demands for accommodation increase, houses originally built with considerable open space behind them, are covered up behind by the erection of other buildings as near as to allow a little light, by not building up windows, so close as completely to exclude any fresh air. The back buildings are often occupied by workshops or immense numbers of bed-rooms and other apartments; and by the processes carried on in them often tend to vitiate completely air by no means pure at its best. There is scarcely one street in London in which the back spaces are not built up more or less completely in this manner; and it is not much better in Manchester and Glasgow. Even in Edinburgh the same process of progressive occupation of free open space is advancing rapidly.

How this evil is to be counteracted it is difficult to say. It is clear that it is not possible to interfere with the private rights of proprietors and others, who conceive that they have a title to make as much money out of their property as possible. Indeed, in all sanitary measures, this difficulty, arising from the love of making money, is incessantly presented.

The recommendations as to drainage and cleaning of sewers and drains ought to be enforced as perfectly as possible.

"We now beg leave to submit the summary of our conclusions in this our first report.

"Having consulted, in relation to the rise and spread of cholera, the experience obtained in the most severely visited districts in the metropolis, and also the most authentic records of the experience in the principal towns in Great Britain and in Europe, we find, in relation to this disease:—

"That amidst the town populations the cholera visits with most severity the same classes of persons and the same places, and is governed nearly by the same circumstances as typhus.

"That it has been proved by experience that those circumstances are generally removable by proper sanatory arrangements, and that typhus is, to a great extent, preventible; and we have every reason to believe that the spread of cholera is preventible by the like means—namely, by general and combined sanatory arrangements.

"That these arrangements, instead of being incidental and collateral to other measures, are paramount and principal, and effective, not only against cholera, but also against other epidemics.

"That when cholera first appeared in this country the general belief was that the disease spreads principally, if not entirely, by communication of the infected with the healthy, and that therefore the main security of nations, cities, and individuals, consists in the isolation of the infected from the uninfected—a doctrine which naturally led to the enforcement of rigorous quarantine regulations; the establishment of military and police cordons; the excitement of panic; and the neglect, and often the abandonment of the sick even by relations and friends.

"That since opportunities have been obtained of a closer observation of the character of this disease, and of the mode in which it spreads through continents, nations, cities, towns, and families, facts have been ascertained which are incompatible with the foregoing view of its mode of dissemination and of its prevention.

"That the disease is not, as it was then generally supposed to be, contagious, and that the practical application of that doctrine did no good, but was fraught with much evil.

"That when it previously visited this country it was believed that the most powerful predisposition to this disease is induced by improper or deficient food, and that, for this reason, its chief victims are found among the poor; but it is now universally admitted that a far more powerful predisponent is the habitual respiration of an impure atmosphere; that the highest degree of susceptibility is produced where both these conditions are combined, that is, where people live irregularly, or on unsuitable diet, and at the same time filthily; and that in places in which a great degree of cleanliness is maintained, the poor as well as the rich enjoy exemption from this disease.

"That on an examination of the actual state of the back streets, lanes, courts, and alleys of the metropolis, it is found that in general little or no improvement has taken place in their sanatory condition since the prevalence of cholera in 1832; and that were this disease again to break out in the present state of these localities, there is no reasonable ground to suppose that the pestilence would not spread as extensively, and prove as fatal as on its former visitation.

"In regard to this disease we fear that complete measures of prevention must be eventual on the combination of works, which must be the subject of further investigations: but in respect to the immediate and special measures available for its prevention, we find that such would be—measures of cleansing of whole lines of sewers, from their commencement, through the several districts to the outfalls; the cleansing of cesspools (wheresoever it may be effected into the sewers), and the removal of whatsoever may be removed in suspension in water in the various modes of flushing, by the use of additional and abundant supplies of water, and we find

"That it is expedient that a commission for the entire drainage of the whole of the metropolis should be appointed, with a special view to such measures, and with aid to carry them out.

"With respect to measures of alleviation of cholera, we find—

"That it is one of the peculiar characteristics of this disease, that it sets at defiance, to a great degree, the resources of medical art and science, as is too fully proved by the fact that, under the most favourable circumstances, of those whom it attacks there perish nearly two out of seven, and under the most unfavourable circumstances nine out of ten.

"That still there can be no doubt that individuals are saved, who would otherwise perish, that are early placed under favourable circumstances and judicious medical treatment.

"That although the removal to cholera hospitals, unless at a very early period of the attack, and unless the situation of the hospital happened to be highly favourable, was proved by experience to be injurious rather than beneficial; yet among the classes most subject to this disease there must be individuals in a state of such utter destitution as to render some provision absolutely necessary.

"That it is desirable that existing establishments for the reception and treatment of the sick be immediately inspected by officers especially conversant with warming, ventilation, and other structural arrangements, to advise on the alterations and adaptations necessary to afford effectual aid to the individuals who may require it.

"That where there is at present adequate accommodation by proper hospitals for fever cases, such accommodation will in general suffice for cholera, fever not being prevalent when cholera is epidemic, and fever cases being in general more numerous than those of cholera.

"On the inquiries we have made as to the administration of the law of sewers in the several districts in the metropolis since the year 1834, when the administrators of that law were examined by a committee of the House of Commons: since the year 1842, when they were examined under the Sanatory Inquiry, and 1843, when they were investigated by the Commissioners of Inquiry into the means of Improving the Health of Towns; and on the best consideration we have been enabled to give to the descriptions of works which have subsequently been carried out by direction of the Courts of Commissioners of Sewers, to the mode in which they have exercised their existing powers, and to the measures for which they have obtained, or propose to obtain, the sanction of parliament, we find—

"That unnecessary expense and inconvenience to the public is consequent on the division of the natural drainage areas among several distinct authorities, and that it is impossible that improved works of systematic drainage can be carried out under arrangements that geographically divide the lines of watershed and the outfalls between separate and conflicting authorities.

"That the works which the present district commissions execute, and propose to execute, are uncertain, erroneous, and defective in their general principles of construction, injurious in their action, and unduly expensive.

"That after the authentic expositions which have been given of the principles of construction and management of improved works, the extensions of sewers or drains, accumulative of decomposing refuse, are acts of injury to the public health and of waste of the public money.

"That the execution by the district courts of commissioners of large works of drainage and sewerage, without reference to any general plan or survey, involves great risk of erroneous and imperfect works and waste of the rates they are empowered to levy.

"For the prevention of disease and the saving of health and life, by early carrying out efficient works of drainage, and diminishing the mass of atmospheric impurities, by which the public health is depressed; and, for the prevention of expenditure upon inefficient works, we feel it our duty to recommend an immediate exercise of the powers of the Crown, and

"That the several commissions appointed under its authority, in the metropolis, be recalled with the least possible delay.

"That the law of sewers, now administered by numerous persons in these separate districts, be confided to one body of commissioners for the whole of the metropolis.

"That to ensure executive dispatch, and obviate that weakening of responsibility which arises from its present division amongst large bodies, the commissioners should be limited in number: and competent, through their known attention to sanatory improvement, to select and sustain the labours of paid officers and the execution of works in the attainment of this their proper object.

"Though the practical consolidation which we submit as necessary, and within the power of the Crown—its rights not being, as we believe, barred by any of the local acts—would be a great advance upon the existing state of things as a permanent arrangement, yet we now submit it as a measure of immediate urgency to abate epidemic disease, and to stay waste; at the same time, we regard it as essentially preparatory to further alterations which we propose hereafter to submit for the consideration of your Majesty.

"Should the apprehension of the return of the pestilence prove unfounded, as we fervently hope it may, all measures preventive of that epidemic cannot fail to be successful against fever and other large and fatal classes of disease. If that which may be done within the time, with every aid and exertion in carrying out such measures, be little, as compared with the magnitude of the evil, it can scarcely fail to repay the effort.

"Concurrently with the immediate works of flushing and cleansing, which the paid officers may be enabled to carry out, we expect that the first work which a consolidated commission must see the necessity of directing to be proceeded with, would be the general survey by the officers of the Royal engineers, under the direction of the Board of Ordnance. Whatsoever may be the modifications of the authorities having charge of the public works for drainage and cleansing, or the ulterior measures which may be adopted, there can be no reasonable doubt of the execution of the survey being the measure of paramount and most pressing importance. Until the consolidation of the districts which we recommend is effected—until the work of the survey is proceeded with, and further inquiries and experiments based upon it are made, we do not see that it will be practicable to make satisfactory proximate estimates of the extent and expense of the alterations and adaptations practicable in the old and erroneously-constructed works, or the extent of new works which may be required to be done for the purposes in question. Such estimates would be desirable, in order to determine the modifications of the executive agency for conducting the new works required. We apprehend that it is only by such a body as the one we recommend for the whole of the metropolis, that complete general results, deducible from a general survey, may be reasonably expected to be brought out for the future guidance of this branch of the administration.

" We beg leave to represent the expediency of concentrating early the largest practicable force of the Ordnance survey upon the metropolis. It appears that surveys have been made for Liverpool, Manchester, and the other towns in Lancashire and Yorkshire, as enumerated in the evidence of Colonel Hall. If the survey of those depressed towns, and that important part of the kingdom had not been completed, we might have adduced evidence of the expediency even, as regards that population, of completing the survey and plans for the metropolis, in the first instance. It has been generally found that the errors of the works of this class in the metropolis are literally copied and exaggerated in the provincial towns, where it is rare and accidental to meet with any improvements upon them in the provincial towns, which had abundant sources of water supply within reach ; the pernicious system of intermittent supplies have been copied from the metropolis, to the injury of trading companies, the deterioration of the supplies, and at double expense of works to the consumers. The metropolis presents, perhaps, the greatest variety of difficult conditions to be dealt with ; upland districts and lowland marshes with outfalls below high-water mark, densely populated town districts and open suburban districts, with much important agricultural drainage to be considered and provided for in the general arrangement of the drainage of the natural areas. The early improvement of these works in the metropolis cannot fail to be conducive to the extension of the like improvements in the provincial towns.

" The south side of the river, of course, forms of itself a distinct drainage area, and might be the subject of a distinct jurisdiction, but we need not advert to all the considerations, amongst which is the saving to the rate-payers of the unnecessary expenses of separate establishment charges, which render it desirable that it should be brought under one and the same survey and course of systematic improvement, under the same administrative body. There are also engineering grounds for this proposed consolidation.

" Though the natural drainage basin is the least extent of geographical area that can be included under one establishment or set of works with economy and efficiency, more than one basin or natural drainage area may be comprehended with public advantage within one jurisdiction. The eventual combination of works for the supply of water, with works for the application of refuse, and with works of sewerage, indicate the necessity of extending the administrative district beyond the drainage areas to comprehend these related works.

" It might be considered and objected that the details of management for so extensive a district would be numerous beyond the powers of one board to compass, and such would be the case if the same modes of transacting business which are now in use in these district courts were to be continued ; if business which ought to be dispatched from day to day by paid, competent, and responsible officers, giving their whole time and attention to the subject, continues to be reserved and delayed, incurring the expensive attendance of architects and builders until court days held at long intervals, when it is dispatched, unavoidably *en masse*, by the honorary and irresponsible members who casually attend, and who dispose of it in the manner described in the evidence of Sir George Philips and of Mr Hertslet, the clerk to the Court of Sewers at Westminster. On analysing the business of these district courts, meeting weekly, fortnightly, monthly, or quarterly, it appears that a large proportion of it arises from the very defects of their own plans and works, which, under an amended system, will disappear. Those who have paid attention to the dispatch of large amounts of varied business are aware that, up to certain limits, the larger the amount the more complete are the means of classifying and systematising it, the better the real dispatch of it. We shall have occasion to cite as an example, the consolidation under the metropolitan road commission of the administration of the roads formerly administered by a number of local trusts, comprehending the suburban parishes in the metropolis. Under that commission, the roads have been improved, the tolls and the debts reduced, and the business of 100 miles of road transacted satisfactorily with less attendance and consumption of time on the part of the honorary members of the board than was previously required by the defective dispatch of business, by any one of the numerous separate boards under which important improvements were found to be impracticable. It may be averred, that the business of a commission of sewers for the whole of the metropolis may be eventually dispatched better and more expeditiously than the business of one of the single commissions.

"On the commissions of sewers, on which we have felt it our duty to report as above, we find the names of his Grace the Duke of Wellington, of the First Lord of the Treasury, of the Lord Chancellor, the Lord Chief Justice, and of other noblemen and gentlemen of the highest position and talent in the country. As it must have been known that they could not attend to the duties of these commissions, we submit that it is objectionable in principle so to use such names, or to associate them before the public with business in which they can really have taken no part.

"It has appeared to be our duty to state that we have had presented to us ground of exception against one class of appointments to these commissions—namely, that of surveyors, of architects in practice, of builders, traders, agents, and professional persons connected with building operations in their respective districts.

"The more the investigation advances, the more is it apparent that the progressive improvement and proper execution of this class of public works, together with the appliances of hydraulic engineering, cannot be reasonably expected to be dealt with incidentally, or collaterally to ordinary occupation, or even to connected professional pursuits, but require a degree of special study which not only place them beyond the sphere of the discussion of popular administrative bodies, but beyond that of ordinary professional engineering and architectural practice.

"In justification of this conclusion, and to show the evil of the perverted application of names of high general professional authority, we might adduce examples of the most defective works which have received their sanction.

"All the improvements which the public have yet obtained in this branch of public works have been the result of the special and undivided practical attention of well-qualified paid officers, and it appears to us that further improvement must be sought by the same means, and that one of the chief objects of future administrative arrangements must be to secure, protect, and encourage the zealous undivided attention and efficient labour of such officers.

"The urgent causes for expediting this our first report have compelled us to restrict our investigations to the condition of the chief districts in which the powers of the Crown may be immediately exercised for the public protection, by revoking the commissions and altering the gengraphical areas of their jurisdictions. We have, therefore, postponed similar investigations in the city of London, where the commissioners of sewers are appointed under an act of parliament by the corporation, and have also the control of the paving and lighting. For the same reason we have deferred any inquiry into the means of improved water supplies, surface cleansing and paving, the assessment and collection of rates, and the other important topics of inquiry involved in our commission. All which we humbly certify to your Majesty.

(Signed) "ROBERT GROSVENOR.
EDWIN CHADWICK.
T. SOUTHWOOD SMITH.
RICHARD OWEN.
RICHARD LAMBERT JONES.

"Gwydyr House, Whitehall, Nov. 19, 1847."

Report on the Mortality of Edinburgh and Leith, for the months of September, October, and November 1847. By JAMES STARK, M.D., Fellow of the Royal College of Physicians of Edinburgh.—The mortality of Edinburgh for the months of September, October, and November 1847, amounted to 1805,—911 being males, 818 females, and 76 still-born. The mortality of the corresponding period of 1845, was 953, of whom 83 were still-born; that of the corresponding months of 1846, was 1263, of whom 62 were still-born.

The mortality of Leith for the above months amounted to 256,—115 being males, 126 females, and 15 still-born; the mortality of the corresponding period of 1845, was 135, of whom 11 were still-born; that of the corresponding months of 1846, was 258, of whom 11 were still-born.

The difference between this season and last, was in several points remarkable. September of last year was a warm month, with a mean temperature of 59.30 degrees Fahr.—the same, in fact, as the mean temperature of July and August of that year; and the thermometer ranged from 78° to 39°, while the prevailing winds were from the south and west. September of this year, however, was a cold ungenial month, with keen easterly and north-easterly winds, and a mean temperature of only 50.98. The thermometer never rose above 66 degrees, and on the 27th of the month, frost occurred, the thermometer sinking to 31°. In September 1846, there fell 3.35 inches of rain, but only 1.25 inches in September of this year.

The weather which prevailed during October this year and last, was closely alike. The mean temperature, the range of temperature, and quantity of rain being almost identical. The mean temperature of October 1847, was 48°.61, the range of temperature from 68° to 34°, and the quantity of rain 3.48 inches. The quantity of hygrometric moisture was greater, however, during October of this year.

The weather, during November of this year and last, differed in several important points, although the mean temperature was nearly the same in both, viz. 44.53 in 1846, and 45.84 in 1847. In November 1846, the weather during the earlier part of the month was remarkably mild; but afterwards, keen frosty weather, with falls of snow, set in. In November of this year, the weather from the beginning of the month was raw and damp, with frequent fogs, alternating with mild days and a south-west wind. On the 17th and 18th, frost with a dense damp fog, came on, and the same kind of weather re-appeared in even greater intensity on the afternoon of the 27th, and the whole of the 28th day of the month. During the first of these periods, the thermometer fell from 51° to 26°, and during the latter, to 25° Fahr. Only 1.64 inches of rain fell during November 1847, being only 1-10th inch of rain less than the fall during November 1846.

The mortality during the above months, particularly during November, attained a height never before equalled in Edinburgh. During the last seven years, the average mortality (excluding still-births) has been 331 per month, or 11 daily. During the year of the cholera (1832), when the mortality attained a height never since surpassed, the deaths were 438 per month, or 14 daily. During September of this year, however, the deaths were 480, or sixteen daily; during October, they were 521, or 17 daily; while, during November, they attained the unusual height of 728, or 24 deaths daily.

The excessive prevalence of fever among the lower classes and immigrant Irish, has been the chief cause of the high mortality during the months of September, October, and the first half of November. During all that period, the deaths from other causes were not greater than in former years, and had this epidemic not been prevalent, the mortality would have been at a minimum. Influenza, however, suddenly made its appearance on the 18th of November, and within a fortnight, attacked more than a half of the whole population. It is important to note that this disease suddenly attacked great masses of the population twice during the course of November; *first*, on the 18th, and *again* on the 28th day of the month. In both these cases the influenza appeared during keen frost, and an excessively damp, thick fog, which came on rather suddenly after a few days of very mild weather. The disease was therefore clearly connected with and dependent on atmospheric causes. In illustration of this connection may be mentioned the fact, that in the Merchant Maiden Hospital, where there are 86 girls, on Saturday the 27th before the frost with the dense fog came on, there was only one girl slightly colded. By next night, however, the day having been one of keen frost, with very thick wetting fog, upwards of 50 were confined to bed with influenza. In George Heriot's Hospital, where there are 180 boys, on Saturday the 27th only about 12 boys were invaliding from the previous attack of influenza; by next morning 40 were confined to bed with influenza, and the number, by the 29th, had increased to 96. Many similar instances might be adduced, but these may suffice to show the close connection of the influenza with atmospheric phenomena.

The second attack of influenza was the most severe and general, and rendered the schools and seminaries of education so deserted, that most of the large schools were obliged to be closed for a short period. On the 29th November, the day on which so many of the schools were closed, returns were procured from various public and private schools, from which it appeared, that of 5036 children on the rolls of the schools, 2285, or 45 per cent., were absent in consequence of being severely affected by influenza; but many were attending who ought to have been in bed. A greater proportion of the younger children were confined than of the older; for while in the junior classes, in many schools, the absentees were in the proportion of 70 to 75 per cent., the proportion among the most advanced and older pupils did not exceed 30 per cent. It was not a little strange, that the children of the various classes of society appeared to suffer in different proportions. Thus, of 2692 children of the labouring or artizan class, only 1054, or 35 per cent., required to absent themselves from school on the 29th—the remainder appearing quite healthy; while of 1591 children of the middle and upper classes, 988, or 62 per cent. were confined with influenza, and many of those at school were suffering more or less severely from the disease. As a general rule, it was remarked, that the more sheltered the situation the less severely was the influenza felt. This was remarked both in schools and public hospitals. For while children from the exposed parts of both Old and New Town were absent in the proportion of about 60 per cent., those living in the low lying and sheltered Cowgate, were absent in the proportion of about 30 per cent. only. In George Heriot's Hospital, situated on a high and exposed site, 96 out of 180 boys were so severely affected, as to be confined to bed on the 29th November; whereas, in George Watson's Hospital, closely adjoining, but in a low sheltered situation, of 86 boys, not exceeding 20 were very slightly affected, only 5 requiring to be in bed for one day. The other hospitals in the neighbourhood from which returns have been procured, exhibited the same fact, but it is unnecessary to instance them individually.

Though influenza was so exceeding general, it did not of itself materially increase the mortality; but this disorder and its atmospheric causes greatly increased the mortality of all other diseases. So much has this been the case, that from the 18th of November, when influenza first appeared, the mortality daily increased till it reached 61 deaths on the 30th day of the month. In fact, influenza and its atmospheric causes apparently attacked the weak point in every individual, be that the lungs, bowels, or other organs, and hastened to a fatal termination cases which, in ordinary seasons, might have survived for months or years.

During the above months, fever has been greatly on the increase in Edinburgh. Since the beginning of the epidemic in April, the mortality of the disease has been one death out of every 7½ cases (*i. e.* two deaths out of fifteen cases of fever); but since influenza appeared, the fatality of the disease has been increased by nearly one-half. During September the number of deaths from fever amounted to 144; during October, to 209; while during November the number increased to 248. Of these 601 deaths from fever, 349 were males and 252 females. 131 were under 20 years of age, 417 were between the ages of 20 and 60 years, while only 53 were above 60 years of age. During 1846, the deaths from typhus fever were in the low proportion of 46 out of 1000 deaths from all other causes. During the above months, however, the mortality from fever was in the high proportion of 312 out of the 1000 deaths from all causes. This fact of itself shows the frightful prevalence of fever in Edinburgh.

Of the other zymotic diseases, small-pox has been rather more prevalent than usual, 28 cases having proved fatal during the above months. Scarlet fever, which has been rare in Edinburgh for several years, has once more made its appearance, and is on the increase; but the cases, for the most part, have been mild, only 13 having died from it during the above period. Measles and hooping-cough, the other epidemic and contagious diseases of children, have been below their usual average during the above months.

The mortality from diseases of the respiratory organs, amounting to 279, has been somewhat higher during the above months than during the corresponding months of last year, when the number of deaths was 235. Almost the whole excess, however, occurred in the month of November, after influenza made its appearance. Croup greatly increased, indeed was very common during November, particularly after influenza appeared. The average monthly mortality from this cause is 5, but during November the deaths increased to 19.

Bowel complaints, which were so prevalent last year during the months of September and October, in consequence of the heat of the season, &c. were not nearly so common this year. They broke out afresh, however, along with, or rather following the influenza, and have been very prevalent since. From the same cause enteritis has been common and fatal, particularly during the latter portion of November. Last year's monthly average mortality from this cause was 8, but during November of this year 26 cases proved fatal. These were of all ages, from a few days to 80 years; the chief mortality, however, fell on infants, 17 out of the 25 being under one year.

The proportion of deaths among aged persons was not greater than usual during the above months, till towards the end of November; the influenza, however, occurred too late in the month to allow the increasing number of deaths among the aged from this cause to swell materially the bills of mortality for November.

The above remarks on the mortality of Edinburgh apply nearly equally to that of Leith. There, however, the mortality has by no means been so great as in Edinburgh. This appears to be attributable to three chief causes. *1st*, Leith is not exposed to the influx of Irish immigrants; *2dly*, It has no public hospital which admits patients from all parts of the country, whose deaths, like those in the Royal Infirmary of Edinburgh, are reckoned along with the general mortality of the town, —its fever hospital is for the inhabitants of Leith alone; and, *3dly*, Its low-lying situation, on the shores of the Frith of Forth, has apparently rendered the effects of the influenza milder than in Edinburgh or other exposed localities. Even as it is, however, the mortality from fever has been in the proportion of 19 per cent. of the total deaths from all causes; while small-pox, both in proportion to the total mortality and also to the population, has been nearly three times as prevalent as in Edinburgh.

The following is an abstract of the mortality tables of Edinburgh and Leith for the months of September, October, and November 1847, classified according to ages and diseases.

TABLE OF AGES.

Ages.	Edinburgh.						Leith.					
	Septem.		October.		Novem.		Sept.		Oct.		Nov.	
	M.	F.	M.	F.	M.	F.	M.	F.	M.	F.	M.	F.
Still-born,	17	10	13	9	14	13	3	2	1	0	6	3
1 yr. & und.	47	39	35	36	65	50	15	9	2	2	9	10
2 .	24	19	21	20	20	28	3	5	4	3	4	6
5	12	12	18	18	28	27	3	2	2	3	4	4
10 .	5	4	9	7	11	11	3	2	0	2	1	3
15	7	4	2	6	3	4	1	0	0	0	0	0
20 .	15	8	12	7	13	7	0	2	1	2	2	1
30	25	21	38	19	50	50	2	1	2	2	4	5
40 .	27	23	42	25	33	44	5	5	4	5	3	4
50	28	26	47	24	54	35	3	4	4	1	4	3
60 .	25	25	22	31	38	35	3	3	3	5	1	3
70	24	20	20	29	32	34	3	4	6	3	1	5
80 .	12	13	7	12	11	20	4	2	0	4	2	4
90	5	5	5	1	3	11	1	0	1	2	1	3
100 .	0	0	1	0	1	0	0	0	0	0	0	0
Not stated,	4	1	4	3	6	4	3	0	0	0	1	2
	277	230	296	247	832	373	52	41	30	34	43	56
Total,	507		543		755		93		64		99	

CLASSIFIED TABLE OF DISEASES.

Class.	Disease.	Edinburgh.			Leith.		
		Sept.	Oct.	Nov.	Sept.	Oct.	Nov.
I.	Small-pox, . .	6	8	14	9	3	3
	Measles, . . .	8	5	6	2	0	2
	Scarlet fever, . .	2	4	7	0	0	1
	Hooping-cough, .	13	6	18	3	1	2
	Croup, . .	4	6	19	2	2	1
	Thrush, . . .	0	0	2	0	0	0
	Diarrhœa, . .	21	8	19	4	0	0
	Dysentery, .	7	4	2	0	0	1
	Cholera, . .	4	1	2	0	0	0
	Influenza, .	0	1	15	0	0	2
	Remittent fever, .	0	0	1	0	2	0
	Typhus fever, .	144	209	248	12	13	23
	Erysipelas, . .	3	3	8	2	1	1
II.	Hæmorrhage, . .	1	1	3	0	0	0
	Dropsy, . .	9	11	17	2	2	2
	Cancer, . .	3	5	1	2	0	1
	Debility, .	16	15	16	3	0	0
	Other dis. of uncertain seat,	3	4	3	1	1	0
III.	Hydrocephalus, .	13	18	18	8	1	3
	Apoplexy, .	6	6	10	3	0	1
	Paralysis, . .	5	6	12	0	2	1
	Convulsions, .	3	1	0	0	0	1
	Insanity, . .	4	4	5	0	3	1
	Delirium Tremens, .	2	2	2	0	0	0
	Other diseases of Brain,	8	4	5	0	0	1
IV.	Bronchitis, .	3	2	17	0	1	6
	Pneumonia, . .	10	18	14	5	3	4
	Asthma, . .	4	6	8	0	0	0
	Consumption, . .	51	46	73	9	6	5
	Other dis. of respiratory organs,	9	4	14	0	0	1
V.	Heart disease, aneurism, &c.	7	7	9	1	0	0
VI.	Teething, .	16	13	15	1	1	2
	Enteritis, . .	15	14	26	0	1	7
	Tabes mesenterica, .	9	13	12	4	2	2
	Other dis. of organs of digestion	11	7	16	1	0	3
VII.	Kidney and Urinary diseases,	4	2	6	0	0	0
VIII.	Child-birth, . .	6	3	12	0	0	0
	Disease of uterus, &c.	1	0	0	0	0	0
IX.	Rheumatism, . .	2	0	1	1	1	1
	Spine and joint disease,	1	2	0	0	1	0
X.	Ulcer, Fistula, &c. .	0	2	1	0	0	0
XI.	Old age, . .	37	33	37	6	11	9
XII.	Intemperance, .	0	2	2	0	0	0
	Violent deaths and suicides,	5	11	7	3	3	3
XIII.	Still-born, . .	27	22	27	5	1	9
	Causes not stated, .	4	4	5	4	2	0
	Total, .	507	543	755	93	64	99

21 Herriot Row, *8th December* 1847.

Death of Mr Liston.—This eminent surgeon died on Tuesday, the 7th December, at half-past 10 P.M. at his house in Clifford Street, London, aged 53 years.

Mr Liston was born in 1794, and was the son of the Reverend Henry Liston, minister of the parish of Ecclesmachan, in the county of Linlithgow—a gentleman who possessed great knowledge of the theory of music, and published a Treatise on Perfect Intonation. After passing through the usual course of education, scholastic and academical, in Edinburgh, Mr Liston began the study of surgery in 1810, by becoming a pupil of the late Dr John Barclay, then an eminent and popular lecturer on anatomy. To this gentleman Mr Liston became eventually assistant and prosector; and it was in this situation, which he occupied till 1815, that he acquired that accurate knowledge of surgical anatomy, and that practical dexterity in the use of the knife, and surgical instruments in general, which speedily raised him to the first rank among operators. In 1815, Mr Liston became surgeon's-clerk, or house-surgeon in the Royal Infirmary, first under the late Mr George Bell, and afterwards under Dr Gillespie. His position in this institution furnished him with the best opportunities for studying the phenomena and pathology of surgical diseases; and as all the morbid inspections were, at that time, performed by the house-surgeons, he enjoyed excellent means of maintaining and improving his knowledge of relative anatomy.

In the course of 1816 Mr Liston went to London, and studied for several months at St George's Hospital. In 1817 he returned to Edinburgh, and commenced practice as a surgeon, while at the same time he taught anatomy, first by giving a course of demonstrations, afterwards both by courses of demonstrations and by lectures, which he continued for several years. During this time, he published a small work on the Surgical Anatomy of Crural Hernia;* and during the same period he performed several brilliant operations;—disarticulating amputations, ligature of arteries, and lithotomy. In truth, Mr Liston was already in great reputation as a lithotomist.

As practice increased so much as to occupy the greater part of his time, Mr Liston withdrew himself from the teaching of anatomy, and confined his attention to lectures on systematic surgery, which were attended by large numbers of pupils. During this period Mr Liston published in successive volumes of this Journal several valuable papers on amputation, difficult cases of aneurism, tracheotomy, the Taliacotian operation, lithotomy, and lithotrity.

In 1828 Mr Liston was appointed one of the surgeons to the Royal Infirmary; and in 1829, he became operating surgeon to that institution; delivering at the same time, first in conjunction with Mr Russell, and afterwards with the other surgeons, clinical lectures on the cases treated.

In 1834 Mr Liston went to London, where his connection with University College and Hospital, as Lecturer on Clinical Surgery, and his subsequent career as an eminent surgeon, are too well known to require commemoration here.

The disease under which Mr Liston laboured was aneurism of the aorta.

Mr Liston was perhaps the most dexterous operator of this country, or perhaps of any country. His natural talents peculiarly qualified him for excellence in this department. Not only was his knowledge of anatomy most correct, and his manual dexterity great, with singular firmness and precision of action, but his mechanical knowledge and his readiness and resources, under the most varied and difficult combinations of circumstances, showed him to have been born almost to excel in operating. These faculties appeared to great advantage in irregular operations, in conducting which he was never at a loss, but showed that he knew exactly what ought to be done and how it was to be accomplished, without delay and without hesitation.

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* Memoir on the Formation and Connection of the Crural Arch, &c. By Robert Liston, Fellow of the R. C. of Surgeons, L. and E. Edinburgh, 1819.

THE

EDINBURGH

MEDICAL AND SURGICAL JOURNAL.

1. APRIL 1848.

PART I.

ORIGINAL COMMUNICATIONS.

ART. I.—*On the Collateral Circulation in Cases of Obliteration or Obstruction of the Venæ Cavæ.* By C. H. HALLETT, Demonstrator of Anatomy in the University of Edinburgh.

A LARGE number of cases, in which the two great venous trunks have been more or less completely obstructed or obliterated, have been placed on record in the numerous journals on medical science, and in various works on medicine and pathology. Yet, in consequence of the lesion not having been detected until after death, and the adjacent parts had been destroyed or considerably damaged by the scalpel, or in consequence of other circumstances usually noticed in the details of the cases,—one of the most important and most interesting points in their history, to the pathologist and anatomist, namely, the manner in which the collateral circulation was established, so as to compensate for the partial or complete impermeability of either of the principal venous trunks, has in many cases entirely escaped observation, in some others has been merely conjectured, and in a few has been only partially disclosed. Our information on this point being very defective, I have thought proper to bring under the notice of the profession, the following instance of complete obliteration of the *vena cava inferior*, together with the right common iliac vein, caused by the inflammation of the coats of these vessels at some considerable period of time before the death of the individual, chiefly on account of the lesion having been early detected in the course of the dissection of the body, and the channels through which the blood returning from the lower half of the body reached the heart, having been clearly made out, although there are some other circumstances connected with the case which render it one of interest.

Towards the latter part of the last winter, the body of a female, aged 58, was brought to the dissecting-room of the University. The body was soft and flabby; the skin was much wrinkled and quite lax over the greater part of the trunk and limbs; yet, immediately after the incisions preliminary to dissection were made in it, a considerable quantity of serous fluid oozed through them, and continued to drain away for some days. It was computed at the time, that not less than four gallons of fluid escaped from the body into utensils placed to receive it, besides that which must have been dissipated by evaporation. The superficial veins were not enlarged, at least not to an appreciable extent, but their coats were observed to be thickened, and at the same time to have lost their elasticity and firmness; for they readily tore on being stretched, and in some instances broke down under the pressure of the dissecting forceps. These circumstances, combined with the fact that the muscles were almost white, in consequence of the small amount of blood, and the large quantity of serous fluid which they contained, led to the dissection being more cautiously conducted than usual, that the cause of these changes might be detected. This was ultimately done by Mr Keith, an able and diligent dissector, who observed a part of the obliterated *vena cava inferior*, and several large and apparently anomalous veins about the posterior parietes of the abdomen. The veins of the trunk were then filled with injection, and after having been carefully dissected, were found to have the following arrangement.

The *vena cava inferior* was pervious, and of its natural size from the heart downwards to the renal veins. Immediately below the renal veins, it was found considerably constricted, having its calibre gradually diminishing for near half an inch. From this point downwards, it was completely obliterated, the vein being converted into a dense, thick, fibrous cord, impervious to the matter of injection and to air forcibly impelled. This fibrous cord was firmly adherent throughout the whole of its course to the walls of the aorta. It also presented several enlargements along its internal border. The first of these enlargements existed at the upper end of the cord where it joined the renal veins, and marked the original diameter of the *vena cava inferior* at this point before the lesion had supervened. Two others were met with towards its lower extremity; and these corresponded to as many constrictions in the aorta, and to the intervertebral cartilages between the three lower lumbar vertebræ.

The canal of the right common iliac vein was also completely obliterated throughout the whole of its extent, and the vein itself converted into a fibrous cord, similar in character and appearance to the obliterated *vena cava.* This cord also presented, on its internal border, an enlargement corresponding to a constriction in the right common iliac artery, to which it was firmly adherent.

The central portions of the right and left renal veins were considerably enlarged, and together constituted a large venous trunk which passed transversely across the aorta and the vertebral column. The trunk formed by their union presented a constriction a little to the right of the aorta. This constriction appeared to mark the division between the two renal veins, which had evidently been closely approximated by the contraction and drawing together of the sides of the *vena cava inferior,* at the time when the process of obliteration was going on. The left renal vein was by far the larger of the two. A little to the left of the aorta, this last mentioned vein was joined by a vein fully equal in size to itself. This vein, which was found to be the upper portion of the left ovarian vein greatly dilated, proceeded downwards for about three quarters of an inch, and then received two vessels of nearly equal dimensions. It was through these two veins that the collateral circulation was carried on, each of them having free and extensive communication with the iliac veins of both sides. The arrangement and communications of these two veins will require to be described separately, inasmuch as they followed a different course, and, as will immediately appear, were in some measure differently concerned in maintaining the collateral circulation.

The outermost vein, nearly half an inch in diameter, when fully distended with injection, was the proper continuation of the ovarian vein. It descended towards the pelvis, describing a considerable curve in its progress downwards, was divided into several smaller branches, which are all more or less tortuous, and reached the left angle of the *fundus uteri*, through the broad ligament of that organ. Here the smaller branches which united to form it were separated into two distinct sets; one of these passing over the anterior surface and in the substance of the uterus, while the other was similarly related to the posterior surface and walls of that organ. The anterior branches dividing and subdividing, and uniting with branches connected through the right uterine vein with the right internal iliac vein, constituted the anterior uterine plexus, which was much dilated and enlarged. The communication with the right iliac vein was established more especially by two veins which were larger and more superficial than the rest. Both these communicating vessels coursed in an oblique direction, and in a slightly tortuous manner, over the body, to the left side of the neck of the uterus, receiving several of the smaller branches in their course. At the *cervix uteri* they united with each other, and with some larger vessels from the posterior surface of the uterus. The posterior set of vessels, which were much larger than the anterior, formed, with the assistance of both uterine veins, the posterior uterine plexus, which was chiefly developed about the neck of the uterus. These vessels united and then joined the anterior set of vessels, the whole combining to form a single large trunk, consisting of the right uterine and right internal iliac vein, which passed on to, and entered the right external iliac vein.

It may be remarked in passing, that the uterus, which was somewhat enlarged and mis-shapen, consisted almost entirely of an immense plexus of veins connected with the uterine and ovarian veins.

The innermost vein, little larger than that already described, applied itself at first to the aorta, and subsequently to the left common iliac artery. It was more tortuous than the ovarian vein, and was twisted and coiled on itself in a peculiar manner. From its termination in the upper portion of the ovarian vein, to its commencement opposite the sacro-iliac synchondrosis, it presented no less than nine different curves, most of which were very abrupt. From its inner surface, and towards its lower part, three veins passed off, which crossed the vertebral column behind the aorta; the lowest of these three veins joined the enlargement already alluded to as existing on the inner surface of the obliterated *vena cava*. Below, this vein received the external and internal iliac veins of the same side, and also the middle sacral vein, which was two or three times larger than it is normally, and inosculating with some enlarged lateral sacral veins, established a very free communication with the right internal iliac vein.

Some difficulty was experienced in making out the exact nature of this vein, which was almost as large as the *vena cava inferior* itself in the normal state. The lower part of the vessel was evidently formed by the enlargement of the lower two-thirds of the left common iliac vein, the upper third of which passed onwards in its usual direction to the *vena cava* where it was abruptly closed. The upper, and by far the greater part of the vessel was at first thought to have been formed by enlarged lumbar veins, but these being afterwards disclosed to view, such an opinion was no longer tenable. Subsequent investigation appeared to show, that this portion of the vessel originated in the enlargement of a very small vein, so small indeed, that in the normal state it will scarcely admit a surgeon's probe within it. It is not perhaps generally known, that in the male subject there usually exists a small vein which passes and establishes a communication between the common iliac vein and the renal vein.* On the left side the communication between these two vessels is indirect and is effected through the left spermatic vein, at least in a large proportion of subjects. The same communicating venule is found in the female, and has the same general arrangement, substituting the ovarian for the spermatic vein. It is not always present;—or rather it would appear sometimes so small that it can be scarcely detected; —but I have satisfied myself, by numerous and repeated dissections, that it may be considered normal. Now, it is this minute communicating vessel, enormously enlarged, that constituted the upper and more tortuous portion of the inner vein that joined the left ovarian vein in this particular case.

The left external iliac vein was simply enlarged. The internal iliac vein was not only enlarged but tortuous. The uterine branches were more than double their usual size, and even larger than I have seen them in females at the full period of utero-gestation. They communicated freely with the posterior larger branches of the left ovarian vein, and established an important channel of communication with the iliac veins of the right side, by means of the posterior uterine plexus.

The lateral sacral veins, two in number, established a large and direct communication between the two internal iliac veins.

Besides the vessels already described, there were others which, from their increased size, must have assisted in conveying the venous blood from the lower extremities and lower parts of the trunk to the centre of the circulation. The left lumbar veins especially, were somewhat enlarged, and formed a considerable trunk that commenced by two branches from the left common iliac vein, and the enlarged vein of communication between it and the ovarian vein. The trunk formed by their union and enlargement bifurcated above into two branches, one of which curved forwards and entered the enlarged communicating vein, while the other was continuous with the *vena azygos minor*, which was but slightly enlarged.

The intra-spinous veins,—*meningo-rachidian*,—had free communication with the lumbar veins, and were somewhat enlarged, especially those on the left side.

Having described the veins which seemed from their increased size and connections to have been principally concerned in establishing the collateral circulation, I may now briefly advert to the manner in which this appears to have been performed. It will be evident at once, that the venous blood returning from the left lower extremity, and from the left half of the pelvis would pass on to the upper portion of the *vena cava inferior*, and thence to the heart, by a channel only a slight degree more tortuous and indirect than that through which it had to pass in the normal and healthy condition of the vascular system. The blood returned to the heart from these parts through the left common iliac vein, the enlarged communicating vein between it and the ovarian vein, and through the renal veins and upper portion of the *vena cava inferior*. The venous blood, however, returning from the right lower extremity and from the right half of the pelvis, would have to pass on to the heart through a very tortuous and indirect channel. It must have found its way from the right external and internal iliac veins, through the uterine plexuses and the lateral sacral veins, into the left internal iliac vein, and into the greatly enlarged left ovarian vein. That part of the blood entering into the left internal iliac vein must have been carried onwards to the heart through the channel apparently chiefly developed to convey back the blood from the left lower extremity and half of the pelvis. And that finding its way into the left ovarian vein—which from its great size and more extensive communications, as well as affording, in some measure, the more direct course, must have taken by far the greater proportion—again mingled with the rest just as it was about to pass into the left renal vein.

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* This vein passes sometimes over the surface, sometimes through the substance of the *psoas magnus*, and has generally communications with one or more of the lumbar veins. It has been figured in Masse's Hand-Atlas, but comparatively of too large size.

The enlarged lumbar veins and the intra-spinal veins, from their comparatively small size, would appear to have been of secondary importance in carrying on the collateral circulation; yet, the fact of their being enlarged, shows that they must have been concerned, in however slight a degree, in conveying the blood from the left iliac veins to the centre of the circulation.

In connection with the changes which had taken place in the veins, it might not be uninteresting to mention some alterations in the general appearance of the arterial system. All the arteries in the body had undergone considerable dilatation. The aorta was displaced, curved at its lower part, and nearly twice the size it ordinarily is. This vessel, together with the right common iliac artery, presented several enlargements or incipient aneurismal dilatations. Where the enlargements existed, the coats of these vessels were separated from each other by a large quantity of a brownish semifluid substance, which exuded from between them when the coats were punctured. The coats of some of the other arteries were in the same condition.

On taking a general view of the collateral circulation established in this particular case, of complete obliteration of the lower part of the *vena cava inferior*, it became evident that the shortest possible channel of communication between the veins of the lower half of the body and the upper part of the *vena cava inferior* had been taken, and that it was, for the most part, the one which an anatomist, on being told the seat of the obliteration, would have marked out as the most direct and patent. Knowing nature's tendency to economize her means for the attainment of certain ends, when she is forced to deviate from the normal course, the selection of the shortest, most patent, and therefore the best channel for the return of the venous blood from the lower half of the body in this instance, led me to examine and arrange the numerous cases on record of obliteration and obstruction of both the *venæ cavæ*, with the purpose of seeing whether the same channel would be selected in all cases where the circumstances were the same,—whether the channel selected was always that which would be considered anatomically the shortest and best,—and, lastly, if there should happen to be differences in some individual cases, whether those differences did not depend on some of the ordinary channels of communication being involved in the morbid action.

As might have been anticipated, from our knowledge of the determinate course adopted by nature in establishing a collateral circulation when any of the larger arteries are obstructed by disease, by ligature, or by natural causes, the investigation terminated in showing, that the analogy between the arterial and the venous systems, so perfect in all other respects, was fully carried out in this; that, when either of the two great channels for the return of the venous blood to the heart was obstructed, the course adopted by nature in establishing a collateral circulation was determinate, and, according to fixed principles, applicable alike to the venous and to the arterial systems.

As far as I am aware, no anatomist or pathologist has hitherto shown or attempted to show, from the numerous cases on record, the facts just mentioned. It is, however, a subject of some interest to both, as will be seen in the sequel, and therefore it will be unnecessary for me to offer any other excuse for bringing under the notice of the profession cases already familiar to some of its members, arranged in a systematic form, for the purpose of showing on what grounds I have advanced the above important conclusion.

It is unnecessary for me to particularise, in this place, the causes giving rise to obliteration or obstruction of the *vena cava superior* and *inferior*;[*] but it is necessary for me to mention the fact, already well established, that the obliteration or obstruction, whatever its cause, is usually circumscribed, and seldom passes beyond certain boundaries in each particular set of cases. This is not well seen in the *vena cava superior*, in consequence of its comparative shortness, but is fully apparent in the *vena cava inferior*, where we are enabled to determine several different sites of obliteration or obstruction. Now, it will be at once evident, that the channel selected for carrying on the collateral circulation will differ most materially according to the site of the obliteration; and as a full but concise exposition of all the facts requires that these different sites should be first made known, I have arranged them as follows:—

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[*] References to most of the cases described in this memoir, together with many others, in which the collateral circulation has not been noticed, are given in the Med.-Chirg. Trans. Vol. x. new series, arranged according to the cause of the lesion.

Vena Cava Inferior.

1. Obliteration or obstruction extending from the right auricle of the heart to the renal veins.

2. Do. do. extending from below the hepatic veins to the renal veins.

3. Do. do. extending from the renal veins downwards.

4. Do. do. extending from immediately above the union of the common iliac veins downwards.

Vena Cava Superior.

5. Obliteration or obstruction either partial or complete, and involving the orifice of the *vena azygos major.*

This arrangement is not altogether complete, since there are some cases on record in which the obliteration is more extended than in those I have taken as the basis of the arrangement. But these constitute the minority, and will therefore be considered with those to which they are most closely allied.

Vena Cava Inferior. 1. *Obstruction or obliteration extending from the right auricle of the heart to the renal veins.*

There is but one case on record in which the *vena cava inferior* was obstructed as high as the right auricle of the heart. The account of the collateral circulation accompanying the notice, however, is imperfect, although enough has been described to render it interesting and worthy of being noticed in this place. The case in question is described by Dr Baillie, and that portion of it connected with the subject of this memoir is detailed as follows:—

"The *vena cava inferior* was found to be changed into a ligamentous substance from the entrance of the emulgent veins, even to the right auricle of the heart. The cavity here was so entirely obliterated as not only to prevent all circulation of blood through this part of the vein, but even, in a great measure, to prevent the admission of air by inflation. The blood being prevented from passing through the *vena cava inferior*, flowed into the lumbar veins, enlarging them gradually as that vein became contracted, till at length they were of a sufficient size to receive the whole blood, which returns by the *vena cava*. From the communication between the lumbar veins and the *vena azygos*, the blood passed into this vein, and was conveyed to the heart. It happened in this particular instance, that there was an additional *vena azy-*

gos upon the left side of the spine, so that the blood was conveyed more readily than if there had been one *vena azygos*, as in ordinary cases. The enlarged veins were in some places thrown into varices, as must naturally take place under the circumstances we have mentioned."*

The blood returning from the lower half of the body in such cases, may be expected to find its way into the lumbar veins and *venæ azygi*, through the lower portion of the *vena cava inferior* and the renal veins, which almost invariably have a considerable communication with the *venæ azygos*, and must therefore be called into requisition where a collateral circulation has to be established under the same circumstances as the above.† It is totally unnecessary for me to dwell on the facts, which must be at once apparent to every anatomist, that the channel opened in Dr Baillie's case was the shortest, and that the enlarged *venæ azygi* will fully compensate for the impermeability of the *vena cava inferior*.

There is one thing, however, entirely overlooked by Dr Baillie, but which, from the words employed in the description, we must take for granted, namely, that the blood returning from the liver to the heart could not have passed through its ordinary channels, since the orifices of the *venæ cavæ hepaticæ* must have been closed by the obliteration of the *vena cava inferior*. Hence arises the question,—through what channel did the decarbonised blood from the liver reach the heart? The solution of this question is not easily arrived at, but it might probably be discovered by attention to the known distribution and communications of the veins of the liver. It has been shown by Professor Kiernan,‡ that the portal system at the liver has free communication with the systemic veins, by means of the capsular veins of the liver, which are a part of the portal system, and anastomose with the diaphragmatic veins. And, moreover, he has shown that these veins form the medium of a collateral circulation in some cases of atrophy of that organ. This being the case, there is every reason to suppose, that the blood being prevented from pursuing its usual and direct course, a more circuitous, but, under the circumstances, the best channel, was opened up for it through the capsular veins of the liver and the diaphragmatic veins, which might have thrown their contents into the *vena azygos*, or some other vein, from whence it could be conveyed to the heart. This, however, is merely hypothesis, and based on the assumption, that the blood did not pass to the right side of the heart through the *venæ cavæ hepaticæ*. I shall, nevertheless, have occasion to point out immediately, that the hypothesis, whether the assumption be true or not, is fully warranted by fact.

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* Transactions of a Society for the Improvement of Medical and Chirurgical Knowledge, Vol. i. p. 127.

† Although Dr Baillie makes no mention of the enlargement of the renal veins, nor of their having taken part in the collateral circulation, yet the plate annexed to the notice shows that they were enlarged, and did participate in the collateral circulation.

‡ Anatomy and Physiology of the Liver. Philosophical Transactions, 1833, p. 733.

2. *Obstruction or obliteration extending from below the* venæ cavæ hepaticæ *to the renal veins.*

It would appear, from the number of cases on record, that the *vena cava inferior* is more liable to be obliterated or obstructed between the hepatic and the renal veins than in any other part of its course. Why this should be I cannot stop to inquire into; but it is necessary for me to mention, that it is here that the greatest variety in the collateral circulation is met with, in consequence of the larger veins emptying themselves into the *vena cava inferior*, especially near the place of obliteration, being more or less involved in the morbid action and changes. Another circumstance, also, I conceive it necessary to mention, namely, that in a large proportion of the cases recorded, in which the obliteration has been discovered between the hepatic and the renal veins, no notice whatever has been taken of the channel through which the blood passed, and therefore the anatomist is less able to determine whether there are any conditions, which might give rise to the differences in other cases, in which the collateral circulation has been described, independent of that already alluded to, namely, the obstruction of some of the ordinary channels of communication.

Having made these preliminary remarks, I shall proceed at once to detail those parts of the cases which bear on the subject in question. The first of these is described by Mr Wilson, and appears to me to be that in which, under the circumstances, the most direct series of veins have been enlarged for the medium of a collateral circulation.

In this case, not only the whole extent of the *vena cava inferior* was obliterated below the entrance of the hepatic veins, but also the emulgent, spermatic, primary, external, and internal iliac veins, and their larger branches filled with lymph and firm plugs of coagulated blood.

" The anastomosing branches of the veins on the sides and back part of the pelvis were much enlarged, as were also those between the *vena saphena major* and the branches accompanying the deep-seated arteries passing through the *foramen magnum ischii* and the sciatic notch. Large communications were seen filled with fluid blood between the *venæ pudicæ externæ* and the lower branches of the *vena mesenterica inferior*, which was enlarged to treble its natural size. The veins coming from the sinuses of the *dura mater* in the *theca vertebralis*, the sinuses themselves, and the veins entering them, were much enlarged; and the communications between them and the sacral and lumbar veins were, by the blood contained in them, rendered very apparent. The enlarged branches of the lumbar veins formed such easy communications with each other as to allow a passage through them to a very large quantity of blood, which entered the *vena azygos* by the anastomosing branches of its lower part. This vein, although three times larger than it commonly is, was without the varicose appearance described by Dr Baillie. The emulgent and phrenic veins communicated largely with the lumbar veins and the *venæ azygi*. The blood passed from the *venæ pudicæ externæ* into the inferior mesenteric vein, and from thence to the *vena porta*; it circulated afterwards through the liver, and entered the lower part of the auricle of the heart, by the very small portion of the *vena cava inferior* which remained pervious, namely, between the auricle and the *venæ cavæ hepaticæ*. The remaining part, having reached the *vena azygos*, passed from it into the *vena cava superior*, and entered the same auricle from above."*

Before offering any remarks on this interesting case, I shall give an abstract of others in the order which I conceive to be best adapted to prepare the reader for the inferences which can be legitimately drawn from them. With this purpose in view, I have now to direct attention to the following statements of Dr T. P. Peacock.

He says that, on removing the uterus from the body of a woman who had died with very clear symptoms of anasarca, "the veins in its substance, in the broad ligaments, and around the ovaries, were found distended by firm fibrinous clots, and on following the venous branches in the course of the circulation, the common iliac veins displayed similar coagula, and they thence extended continuously throughout the cava till about an inch and a half below its passage through the diaphragm, that vessel became reduced to an impervious ligamentous cord. The fibrinous clots on the right side, though firmly obstructing the common iliac vein, ceased immediately above its division; but on the left side they extended both into the hypogastric and external iliac veins, entirely closing their cavities."

"The communication of the left emulgent vein with the cava was open, and this vessel was thoroughly free from coagula; a vein of considerable size, also unobstructed, entered it from above. The right emulgent vein was closed by a large and closely-adherent coagulum, projecting into its orifice. On examination, the *vena azygos* was found of unusually large size, being immediately above the entrance of the *vena azygos minor*, fully one inch in circumference; the branches entering the right vein and the *vena azygos minor*, with its branches, and especially some from the spinal canal and intercostal spaces, were greatly enlarged. The portal trunk and branches were free from disease. Though the convex surface of the liver was covered by enlarged veins, the diaphragmatic veins were not materially dilated. The veins of the integuments of the abdomen, and those following the course of the internal epigastric artery, did not appear larger than usual."*

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* Transactions of a Society for the Improvement, &c. Vol. iii. p. 70.

A very remarkable and interesting case is detailed at full length by the distinguished French pathologist, Reynaud; and, as it bears on several different points connected with the subject of this memoir, I shall transcribe the history of the autopsy in his own words.†

"The body was inspected the following day. We expected that we should find an obliteration, more or less complete, of the *vena*

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* Medico-Chirurgical Transactions, Vol. x. new series. p. 4, et seq.

† "L'autopsie cadavérique fut pratiquée le lendemain. On s'attendait à rencontrer une oblitération plus ou moins complète de la veine porte ou de la veine cave, etles recherches furent dirigées de ce coté. Voici dans quel état se sont trouvées ces deux veines: la branche droite de la veine porte, ainsi que toutes ses divisions, dans le lobe droit du foie, qui lui-meme se trouvait dans un état d'atrophie remarquable, etaient entièrement obstruées par une matière solide. Cette altération se continuait dans les veines hépatiques correspondantes; au moins les branches de celles-ci et leur troncs étaient-ils oblitérés entièrement par la même substance jusqu'à leur embouchure dans la veine cave, qui elle-même en était remplie dans l'étendue de trois pouces, a partir de son embouchure dans l'oreillette. Tel était l'état des vaisseaux oblitérés.

"La branche gauche de la veine porte était au contraire remplie de sang liquide, et ses branches dans le foie semblaient avoir un calibre beaucoup plus considérable que celui qu'elles ont d'ordinaire. Les veines hépatiques correspondantes participaient à cette dilatation générale, et près du lieu où elles se rendent dans le veine cave, leur calibre etait peut-être triplée. Enfin un tronc, que son état de vacuité devait faire regarder comme ayant dû donné passage au sang pendant la vie, se débouchait dans la veine cave, tout près de l'oreillette et au-dessus du lieu de son oblitération.

"Les énormes veines que nous avions veues pendant la vie, se ramifiant sous les tégumens du tronc, naissent des veines épigastriques et allaient se jeter en partie dans les troncs des intercostales, et en partie dans les veines axillaires; leur volume en etait considerablement accru encore, de même que celui de l'azygos.

"Enfin, une dissection attentive nous fit découvrir quelques autres voies anastomotiques vein propres a demontrer la puissance de l'organisme pour le rétablissement de la circulation, lorsqu'elle vient à être interrompue dans les gros troncs vasculaires. D'assez grosses veines se voyaient sous le feuillet séreux qui recouvre la surface convexe du foie; et après avoir reçu une foule de rameaux vasculaires de toutes les parties voisines, et de l'intérieur du foie lui-même, elles s'abouchaient avec un tronc unique, lequel, après avoir transversé le diaphragme, venait se rendre dans l'oreillette droite. La grand veine coronaire du cœur, faisait elle-même partie de ce grand cercle anastomotique. En effet, une multitude de veines sous-diaphragmatiques, s'anastomosant avec d'autres qui existent à la face supériéure de ce muscle, se continuaient dans l'epaisseur du pericarde au-dessous de son feuillet séreux; et parvenues au point où ce dernier se réfléchit sur le cœur, ou les voyaient se contourner sur le base de cet organe, et se jeter dans la veine coronaire. Aussi ce vaisseaux avait-il un volume approchant de celui de la veine crurale, et son orifice dans l'oreillette se faisait-il remarquer par sa grandeur inaccoutumée. Les veines de l'intestin étaient en général dilatées."

portæ or of the *vena cava;* and our examination was directed to ascertain this point. These two veins were found in the following state:—The right branch of the portal vein, and all its divisions in the right lobe of the liver, which was found in a state of remarkable atrophy, were entirely obstructed by a solid substance. This change was continued into the corresponding hepatic veins; at least the branches of these veins and their trunks were entirely obliterated by the same substance, as far as the opening into the *vena cava*, which was also filled with the same matter for the space of three inches from its orifice in the auricle. This was the state of the obliterated vessels.

"The left branch of the portal vein was, on the other hand, filled with liquid blood; and its branches passing through the liver appeared to present a calibre greatly larger than that which they usually show. The corresponding hepatic vein partook in this general enlargement, and near the spot where they enter into the *vena cava,* their calibre was probably tripled. Lastly, a trunk, which its empty state might cause to be regarded as one which during life gave passage to blood, opened in the *vena cava*, close to the auricle and above the point of obliteration.

"The large veins which we saw during life ramifying under the integuments of the trunk, arose from the epigastric veins, and proceeded to unite, partly with the trunks of the intercostal veins, and partly with the axillary veins. Their size was still considerable, as well as that of the *vena azygos.*

"Lastly, careful dissection showed us some other anastomotic veins, well calculated to demonstrate the power of the economy in re-establishing the circulation when it happens to be obstructed in the large vascular trunks. Veins of considerable size were seen beneath the serous membrane covering the convex surface of the liver; and, after receiving a number of vascular twigs from all the neighbouring parts, and from the internal substance of the liver itself, these ended in one single trunk, which, after traversing the diaphragm, terminated in the right auricle. The large coronary vein of the heart formed part of this great anastomotic circle. In truth, a number of sub-diaphragmatic veins, anastomosing with each other at the superior surface of this muscle, were continued through the substance of the pericardium beneath its serous membrane; and, having arrived at that point, at which the latter is reflected on the heart, they were seen winding over the base of this organ, and passing into the coronary vein. This vessel further presented a volume approaching to that of the crural vein; and its opening in the auricle was indicated by its unusual magnitude.

"The veins of the intestinal tube were in general dilated."*

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* Obliteration de la veine cave et d'une partie de la veine porte. Circulation

Although there is not a case on record with the collateral circulation described, in which the *vena cava inferior* was obliterated or obstructed between the hepatic veins and the renal veins, without some of the principal veins about the abdomen being similarly situated, yet, considered anatomically, the same channel should be employed for conveying the blood from the lower half of the body to the heart, in such cases as in those where the lesion extends above the hepatic veins. The *vena cava inferior* receives no collateral branches above the renal veins, and, therefore, no considerable deviation from the first set of cases is to be expected. Two of the cases just related, although not pure specimens of the lesion, show that the *venæ azygi* bear the principal burden of the collateral circulation, and that the venous blood reaches them through the lumbar veins and the branches freely anastomosing with them, as for instance the meningo-rachidian and other spinal veins. There can be no doubt, however, that if the *vena cava inferior* was not obstructed below the renal veins, or if the renal veins and their tributaries were not obstructed, the greater part of the blood would find its way from them into the *venæ azygi*, and thence on to the heart.

In considering this particular set of cases, it is to be remembered that, under ordinary circumstances, the portal system is unembarrassed, and is therefore capable of participating in the compensating action,—of assisting in carrying on the collateral circulation. From the interesting observations of Mr Wilson, previously related, we observe, that what might have been merely a matter of conjecture is found to be matter of fact; that the portal system, by means of its somewhat extensive communication with the systemic veins in the pelvis, through the radicles of the *vena mesenterica inferior* and the *venæ hæmorrhoideæ*, does assist in carrying on the collateral circulation. Thus the inferior mesenteric vein in Wilson's case was found treble its natural size, in consequence of its having been enlarged to receive a large proportion of the venous blood returning from the lower extremities, through the *venæ pudicæ externæ*, which, however, could only have passed on to it through the intermediate branches of the hæmorrhoidal veins. At the same time, it must be understood, that observation teaches us that the *venæ azygi*, where the branches passing into them are not obstructed, or the lesion more extensive than ordinary, are capable, by enlarging, of conveying all the blood from the lower half of the body to the heart, and that it is only when some of the ordinary channels of communication between the *venæ azygi*, their tributaries, and the veins of the pelvis and lower extremities are

veneuse collaterale. Journal Hebdomadaire de Médecine, vol. v. p 177 et seq. —An abstract of this paper is published in the Lancet for 19th December 1829, page 424.

involved in the diseased action, that the portal system is required to act as a medium for the collateral circulation; and then it is obvious, that, under such circumstances, it affords the most direct and largest channel.

The pathologist may perhaps be inclined to deny the inference now drawn, on the ground that it is supported by one observation only. He will, however, perceive that anatomy teaches us that such conditions may be looked for when the portal system is unembarrassed, and moreover, that they have been observed. The reason why the portal system did not participate in the compensating action in Peacock's and Reynaud's cases, is to be looked for in the circumstance that the vessels of the portal system were obstructed. In Reynaud's case, the right portion of the liver was rendered wholly useless by carcinomatous deposit in the corresponding divisions of the portal veins, and of the hepatic veins also; while in Dr Peacock's case, "the liver was extremely small, weighing only twenty-four ounces;" it was atrophied. Consequently, in neither case was the portal system in a state in which it would be of any great use in carrying on the collateral circulation. It is certainly stated expressly by Reynaud that "the veins of the intestines were generally dilated," but this was most probably caused by the obstruction of the veins at the liver. Indeed, the history of the two cases, as given by these observers, shows that the portal system was so greatly embarrassed as to require an extraordinary channel to be opened up to relieve it. This observation leads me to make a few additional remarks in regard to the channel through which the blood may be expected to pass when the *venæ cavæ hepaticæ* are closed by the obstruction or obliteration of that part of the *vena cava inferior* into which they open.

It has been already stated that Kiernan has shown that in some cases of atrophy of the liver, its capsular veins, which pass into the portal veins on the one hand, and into the diaphragmatic veins on the other, are enlarged so as to carry off the blood from the portal system, and thereby prevent stagnation of the blood and its consequences. The case of Dr Peacock appears to have been one of this kind;—the liver was small and covered by the enlarged capsular veins. In Reynaud's case there was no atrophy, but the same consequences were induced by the greater part of the liver being rendered impermeable to blood by deposit in the veins. This fact is most interesting, because it shows that under any circumstances producing obstruction to the flow of blood through the liver, the course adopted by nature is always the same; that whether the portal circulation be embarrassed by atrophy of the liver, or by obstruction of part of the portal and hepatic veins within the substance of the organ, the same set of veins is enlarged, and forms the medium of a collateral circulation. It will now be seen on what grounds I inferred that if the hepatic veins were closed in Dr Baillie's case, the capsular and diaphragmatic veins had been dilated to relieve the portal circulation. Previous to the beautiful and interesting observations of Mr Kiernan on the anatomy and physiology of the liver, an explanation of the rationale of these cases could not have been given; nor would one of the most interesting conclusions deduced in this memoir have been supported by the strongest evidence.

Why were the superficial and deep veins of the front and sides of the abdomen and thorax enlarged in Reynaud's case? Before I attempt an explanation of this fact, I am necessitated to anticipate some remarks to be made hereafter, by stating that it is contrary to the general rule for the veins of the parietes of the trunk to be enlarged in cases of obliteration or obstruction of the *vena cava inferior*. According to the principle already laid down, this departure from the general rule should be attended with some obstruction of the veins forming the proper medium of the collateral circulation. I am therefore inclined to believe that some obstruction of the lumbar veins or of the *venæ azygi* existed, or that there was some abnormal distribution of those veins. The history of the case does not afford any evidence in support of the former opinion, nor indeed of the latter, probably because the posterior part of the abdominal cavity was not inspected; still, if such an anomaly as the following existed in the individual, whether the veins were obstructed or not, it would readily have accounted for the departure from the general rule in this particular instance. The azygous veins have been seen on several occasions by myself to receive only the intercostal veins, and to have no communication either with the renal veins, or with the lumbar veins which passed into the *vena cava*; so that, had the *vena cava inferior* been obstructed above the renal veins in those subjects, the ordinary channel would not have been engaged in the collateral circulation; some other would have been selected, and that one could only have been found in the veins of the abdominal and thoracic parietes. That such was the case in the instance in question is within the range of possibility; but as we have no evidence of the fact, we must be content to consider the enlargement of the veins of the front and sides of the trunk as an exception to the general rule.

3. *Obliteration or obstruction extending from immediately below the renal veins downwards.*

Obliteration or obstruction of the *vena cava inferior*, immediately below the renal veins, appears only to occur in females, and seems to be connected in some way or another with the pregnant or puerperal state. An inquiry into the cause of this would be interesting; but as it would be irrelevant to the subject under consideration, it is unnecessary for me to do more than mention the fact. The cases recorded of the lesion existing in this portion of the *vena cava inferior* are far from numerous, but all of them exhibit, in every essential respect, the same character,—a circumstance which first led me to investigate the whole subject. The case described at the beginning of this paper is one of this kind; and as it has been already detailed at full length, it will only be necessary for me to direct attention to the essential points in the collateral circulation.

"The blood returning from those parts—the left lower extremity, and left half of the pelvis—would pass on to the upper portion of the *vena cava inferior*, and thence to the heart, by a channel only a slight degree more tortuous and indirect than that through which it had to pass in the normal and healthy condition of the vascular system. The blood returned through the left common iliac vein, the enlarged communicating vein between it and the ovarian vein, and through the renal veins and upper portion of the *vena cava inferior.* The venous blood, however, returning from the right lower extremity, and from the right half of the pelvis, would have to pass on to the heart through a very tortuous and indirect channel. It must have found its way from the right external and internal iliac veins, through the uterine plexuses, and the lateral sacral veins, into the left internal iliac vein, and the greatly enlarged ovarian vein. That part of the blood entering into the left internal iliac vein must have been carried onwards to the heart through the channel apparently chiefly developed to convey back the blood from the left lower extremity and half of the pelvis. And that finding its way into the left ovarian vein,—which, from its great size and more extensive communications, as well as affording, in some measure, the more direct course, must have taken by far the greater proportion,—again mingled with the rest, just as it was about to pass into the left renal vein." So that, to speak in more general terms, the ovarian vein, or, as it is sometimes called, the spermatie vein, and the branches of communication between it and other veins, was the principal medium of the collateral circulation.

The next case is briefly described by Haller in the following words:—

In a female of 40 years, I found the *vena cava* between the origin of the left renal vein and the iliac veins greatly narrowed, so that it transmitted scarcely anything. Some firm fibrinous blood, however, was found within its cavity, which was most straitened between the thick membranes. The right spermatic vein, greatly enlarged, entered the *vena cava* with the diameter of one inch, and conveyed the blood of the ureteric vein and others, which are in the sound body small, arising from the right iliac vein.*

Another case, similar in its general bearings, had been seen and noticed by Rhodius. I have been unable to consult the original description, and am therefore obliged to mention it, only as referred to by Haller, who says, in continuation of the report of his own case, "rarissimi hujusmodi coalitus exemplum aliud reperi in Johannis Rhodii Mantissa Anatomica."†

In all the instances of obliteration of the *vena cava inferior*, immediately below the renal veins, the ovarian or spermatic vein of one or other side was the principal medium of the collateral circulation. In the two sets of cases previously described, the *venæ azygi* were the chief agents; but in this we find, that in pursuance of the principle that the shortest and most patent channel is always selected, the *venæ azygi* remain almost in their natural state, while one or other of the ovarian or spermatic veins and its communications are opened up to convey the blood into the upper and pervious portion of the *vena cava inferior*. That the ovarian veins afford the most direct and patent channel does not admit of a doubt. The communications between the veins of both sides of the pelvis are at all times considerable, but are rendered much more so by the extensive enlargement of the calibre of the uterine and ovarian veins, which form the most important of the venous plexuses connecting the larger veins of the two sides together during pregnancy.

There is no evidence of the size the ovarian or spermatic vein had attained in the case recorded by Rhodius; but in my own, and in Haller's, it was fully as large as the *vena cava* itself usually is in the generality of subjects, so that we might presume it was equally enlarged in the other case. There was consequently no necessity for the enlargement of any other set of veins, as the *venæ azygi* or the portal veins, either or both of which could participate in the collateral circulation, if in any case the ordinary and proper channel happened to be obstructed.

It has been already stated that all the individuals in which the *vena cava inferior* has been found obliterated from immediately below the entrance of the renal veins into it were females. Had they been males, the channel selected would have been somewhat different, in consequence of the spermatic veins having, except in some few cases, but slight communication with the pelvic veins, or with the iliac veins. Still there is every reason to believe that the pelvic veins more especially would be enlarged to the same degree as in the female, but the blood would have to pass from them to the heart, either through the communicating vein between the lumbar veins and the renal veins, or through the *venæ azygi*, or, in some cases, where the communication was more extensive than ordinary, through the spermatic vein. The anatomist will see that the best and shortest channel has been selected in cases where the obliteration has been observed in the female, and therefore, if an obliteration in the same site were to occur in the male, we should expect the same principle to be carried out, although the difference in the position and arrangement of the organs from which the spermatic or ovarian veins pass off, would almost unavoidably give rise to a slight modification in the character of the channel selected.

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* "In fœmina quadragenaria reperi venam cavam inter renalis sinistræ originem, et inter iliacas venas, enormiter angustatam, ut vix quid quam transmitteret. Aliquid tamen polyposi duri sanguinis in ejus cavitate, quæ inter crassas membranas arctissima fuit, repertum est. Vena vero spermatica dextra enormiter dilatata, unciali diametro venæ cavæ locum subiit, et sanguinem, exclusum a via solita, reddidit ureteris venæ, aliique in sano corpore exiguæ, ortæ ab iliaca dextra."—Philosophical Transactions, vol. xliv. 1746 and 1747, p. 527.

† Observation xxi.

4. *Obliteration or obstruction immediately above the union of the common iliac veins.*

There is a case of this kind noticed by Wishart, as seen by Mr Cline, in which, although the obstruction existed immediately above the union of the common iliac veins, circumstances were connected with it which evidently prevented the natural and proper channel from being opened up to the extent required. The *vena cava inferior* was found obliterated a little above its bifurcation, in consequence of the pressure of a steatomatous tumour formed in the cellular membrane behind the peritoneum. The tumour occupied part of the pelvis and of the loins. The collateral circulation is described as follows:—"The epigastric veins were become as large as the little finger, and the superficial veins of the abdomen, as well as the lumbar, and those of the internal cavity of the abdomen, were in a similar manner very much dilated; the internal mammary vein likewise greatly enlarged, and also the epigastric, with which it anastomosed, opened as usual into the superior *vena cava*, near to the origin of the subclavian veins; by which circumstance the venous blood of the lower extremities was poured into the superior cava by means of the mammary vein, and into the inferior cava by the lumbar veins, above the compression caused by the steatomatous tumour."*

There can be no doubt, that the principle regulating the formation of a channel for a collateral circulation in cases of obliteration of the *vena cava inferior*, in its upper and middle portions, is in full force here. If the obliteration or obstruction were confined to the *vena cava*, there is no reason why the lumbar veins should not be enlarged to an extent sufficient to convey the whole of the blood from the lower extremities and the pelvis to the

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* Wishart's translation of Scarpa's work on Aneurism, p. 20, note.

vena cava immediately above the point where the obstruction ceases. This would be the shortest and best channel. In the case related by Wishart, the cause of the obstruction extended into the pelvis and involved the parts about the loins, so that part of the veins, which would have been enlarged had the cause been confined to one spot, and that the lowest part of the *vena cava*, were compressed, at least to some extent. The ordinary and proper channel must have been embarrassed, and it is in consequence of this that the veins of the anterior parietes of the trunk were enlarged as well as the lumbar veins. From the terms made use of in describing the collateral circulation, although they are somewhat vague and not anatomically correct, it seems to me that the lumbar veins conveyed the blood from one extremity to the *vena cava inferior*, while the abdominal and thoracic veins carried the blood from the other to the superior *vena cava*. At any rate, the enlargement of the veins of the anterior abdominal and thoracic parietes, in cases of obliteration or obstruction of the *vena cava inferior*, is an exception to the general rule, and was caused by part of the ordinary channel being embarrassed by the pressure of the steatomatous tumour.

Having finished all that appears essentially necessary to be said with regard to the collateral circulation in cases of obstruction or obliteration of the *vena cava inferior*, I have now to pass on to the consideration of the subject as connected with similar lesions of the *vena cava superior*. The cases of the lesions recorded in various works are numerous, but in very few of them has the whole of the veins engaged in carrying on the collateral circulation been described. I shall therefore select those in which the greatest number have been described, and by placing them in connection with each other, attempt to deduce a general rule regarding the medium of the collateral circulation in such cases.

5. *Obliteration or obstruction, either partial or complete, and involving the orifice of the vena azygos major.*[*]

Dr Reid notices a case in which the *vena cava superior* was obliterated for about two inches from its entrance into the right auricle. This circumstance was not discovered until the dissection of the body was far advanced; but after much careful examination, some of the channels through which the blood passed to the *vena cava inferior* were made out. He says, that "on examining the *vena azygos*, it was found to be at least twice its usual size, and became suddenly impervious where it joined the oblite-

[*] There appears to be a set of cases in which the descending cava is obliterated or obstructed between the orifice of the *vena azygos major* and the right auricle of the heart; but I have not been able to arrange them so as to see whether there is any difference in the medium of the collateral circulation.

rated cava. The right intercostal veins, particularly the superior, were much enlarged, and a vein of the size of a common quill passed between the cava immediately before its obliteration and the upper part of the *vena azygos.* The *vena azygos* was joined at its usual place, viz. about the seventh dorsal vertebra, by what is called the *vena demi-azygos*, which was more than twice its usual size. The state of the superior intercostal veins of the left side could not be ascertained, as they had been nearly all destroyed; but to judge of their size from some remnants of them, they must also have been considerably enlarged. On blowing air down the *azygos* and *demi-azygos*, they were observed to retain their large size as low as the diaphragm, and to form several flexuosities. The air passed freely along the enlarged intercostal veins and superior lumbar veins, distending all the lumbar veins, and filling the *vena cava ascendens.* The lumbar veins were also considerably enlarged. A vein of at least the size of a common writing quill, formed by a branch from each of the azygos veins, passed through the left side of the aortic opening, and joined the *cava inferior* near its junction with the renal veins. The internal mammary veins had been destroyed, but the epigastric veins were of their natural size, so that there could not be any reflux of blood in that direction.*

In an inaugural dissertation, Dr Deckart has described a case in which the *vena cava descendens* was obstructed near its entrance into the heart by an aneurismal tumour. The lesion was not discovered until after the dissection of the body was completed. It was observed, however, that on opening the chest an unusually large quantity of blood poured out of the divided veins, and hence Dr Deckart was inclined to consider that the blood passed from the upper part of the body to the lower, through the internal mammary and epigastric veins. He might have added, through the intercostal veins and *venae azygi* also.†

Otto has noticed a case in which the *vena cava ascendens* was closed from the point of entrance of the *vena azygos* to the auricle. He found the veins of the thoracic and abdominal parietes, especially the left internal mammary vein, as well as the *venae azygos*, much enlarged. The right internal mammary vein was obliterated.‡

In the Lancet there is a case related of a patient under the care of Dr Elliotson, with obliteration of the *vena cava superior,* in which mention is made of enlarged veins being seen during life on the parietes of the abdomen and thorax.§ And Reynaud relates another, in which these veins were found much dilated. But in neither case have we any further clue to the channels through which the blood passed.[*]

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* Edin. Med. and Surg. Jour. Vol. xliii. p. 297.

† Otto's Pathological Anatomy, by South.

‡ Neue seltene Beobachtungen, Berlin, 1824, part i. p. 65.

§ August 13, 1836, p. 686.

Now, taking a general view of all these cases, it will be found that in all essential respects they coincide with each other; that the collateral circulation was maintained by the *venae azygi*, which received the large intercostal veins; and, though not invariably, by the enlarged epigastric veins, which received the equally enlarged internal mammary veins, and the subcutaneous veins of the abdominal and thoracic parietes, which communicated freely with each other, and were in general greatly dilated.[†] No other channel but this could have afforded transit to the large quantity of blood returned from the head, and neck, and the upper extremities. Dr Deckart, however, in the remarks he makes on the case already quoted, considered that other channels might have afforded a medium for the collateral circulation. He supposed that the circulation could have been maintained through the communications of the veins on the two surfaces of the diaphragm, or through the veins and sinuses within the vertebral canal. That the blood might follow either of these courses, no one will deny; but no evidence from his own observations or those of others can be adduced in support of such opinions. Such channels would have been very circuitous; and he therefore very justly gave the preference to the veins of the parietes of the thorax and abdomen, which would convey the blood into the *venae azygi* and epigastric veins.

Dr Hodgson, in his work on diseases of the arteries and veins, makes the following remark:—"The venous, like the arterial system, appears to be capable of carrying on a collateral circulation, when any part of it is impervious. The effects which might be expected to follow the obliteration of a principal vein are obviated by the number of its communicating branches, which, in the natural state, are so arranged as to afford a ready passage for the blood into the superior venous trunks, when an inferior portion of the vessel is impervious. It appears to be through the innumerable communications of the superficial branches that the circulation is carried on when a principal vein is obliterated, although a few channels are ultimately more dilated than the rest." The latter part of this remark, however, is not generally correct. Had Dr Hodgson stated that it was through the superficial branches[‡] the circulation was carried on, when the principal vein of the upper half of the body is obliterated, he would have stated the rule. When the inferior *vena cava* is obliterated, the deep-seated veins are generally enlarged, and the superficial veins are only dilated when a number of other veins besides the *vena cava inferior* are obstructed. Out of all the cases I have been enabled to collect, two only presented the superficial veins enlarged, when the lower cava was obstructed; and in one of these we are enabled to trace the existence of disturbing causes,—causes which prevented the usual channel from being opened up to the usual extent.

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[*] Journal Hebdomadaire de Medecine, vol. ii. p. 110.

[†] It has been seen that the *venae azygi* are capable of transmitting the whole of the blood from the lower half of the body to the *vena cava superior*, when the inferior cava is obliterated. The vein appears also capable of conveying the blood from the upper half of the body to the *vena cava inferior*, when the superior cava is obliterated. Why the epigastric veins should, in some cases, participate in the collateral circulation, I have been unable to determine.

[‡] By conveying the blood to the *venae azygi*, and, it may be, to the epigastric vein.

If we take a more general view of all the cases detailed in this paper than has been taken before, it will be found that the *venae azygi* form the most important medium for a collateral circulation, and that they are most frequently enlarged. This arises from the necessity of the whole of the blood from the upper half of the body being conveyed to the ascending cava, where the descending cava is obliterated; and of the whole of the blood from the lower half of the body being carried to the descending cava, where the ascending cava is obliterated above the renal veins. In the one case, the azygous veins collect the blood from dilated superficial and parietal veins; in the other, they collect it from the deep-seated veins. There is only one other vein which vies with the azygous veins in importance, and that only in a limited and particular set of cases. I refer to the ovarian vein.

Where the *vena cava inferior* is obliterated in any part of its course below the renal veins, it is the usual course for the blood to be poured directly into the cava above the point of obliteration. And even when it is obliterated above the renal veins, and any of the usual channels are obstructed, the tendency appears to be, to open up the portal system of veins, in preference to the veins of the abdominal and thoracic parietes.

The extent to which parts of the venous system can be dilated, under certain circumstances, is certainly very great. This must have been rendered apparent by the perusal of my own and of Reynaud's description of the collateral circulation in the cases which have fallen under our notice. In my own case, I found the minute venous twig which generally passes between the ovarian vein and the common iliac vein, so enormously dilated, that it was nearly as large as the cava ordinarily is. In Reynaud's case, the capsular veins of the liver, which are almost capillary in the normal state, were dilated to a very great extent. This power of enlarging certain parts of the vascular system, when necessary, is thus exercised more extensively on the venous than on the arterial system.

In the preceding pages I have attempted to show, as far as the cases on record would permit me, that, in establishing a collatera circulation, the venous system is governed by the same laws as the arterial system, thus causing a more extended analogy between them than is in general supposed; that in all cases—for the apparent exceptions only seem to support and prove the law—the channel selected is always the shortest and most direct, and is always formed from the anastomoses of those vessels which establish the most patent, and at the same time the most extensive, communication between the upper and lower portions of the vessel, or the upper and lower halves of the body, according to the vessel in which the lesion occurs, or, what is of equal importance, the site of the lesion. A determinate course is thus recognised in nature's operations; and thus, as we are accustomed to reckon on the enlargement of certain anastomosing vessels in ligature of arteries, so might we reckon, and with as much certainty, on the enlargement of a particular set of veins in each case, in cases of obstruction of parts of the venous system, for the maintenance of the circulation.

The evidence I have been enabled to adduce in support of this view may not be deemed by some sufficiently conclusive. All the cases on record bearing on the subject—I have not wittingly overlooked one*—have been detailed. It is for others to determine whether the inferences I have drawn from those cases, and from the known anatomical distribution of the veins, are satisfactory. If satisfactory, I shall consider myself amply repaid for the arduous labour I had voluntarily imposed on myself, by having added a further inducement to the study of the venous system, which, although it takes a most important part in the greater number of physiological and pathological operations, has not received from the profession that degree of attention which, I am led to believe, it really deserves.

Art. II.—*Annual Report on the Health of the Edinburgh Police Force for* 1846. By William Tait, Surgeon to the Establishment.

In the month of March last year, I brought before the Medico-Chirurgical Society a report on the state of health of the Edinburgh Police, and endeavoured to establish, from accurate observations made during the previous year, the inference, that the amount of sickness prevalent amongst that force greatly exceeded that known to exist amongst any other class in the community. If the statistical details then brought forward failed to confirm this proposition, and convey conviction to the mind of every one then present, the additional facts now to be submitted will go far to remove any doubt that may be entertained upon the subject. Why there should be so much sickness amongst the police, is a question not so easily answered, but one, nevertheless, which deserves a patient investigation, and one which we expect in a short time satisfactorily to solve. Many causes were adverted to in my last report as likely, in some measure, to account for it; and it has been my business during the last year to watch the same influences in operation, and to note the results which have followed. These I shall now, as briefly as is compatible with perspicuity, proceed to lay before you, apologizing at the same time for the dryness of my subject, and the attention which will be required to follow me through my numerous and somewhat complicated calculations.

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* While this memoir was passing through the press, I had my attention directed to some additional cases recorded in Watson's Lectures on the Principles and Practice of medicine, which tend to corroborate the general laws deduced from the cases recorded by others, and fully noticed in this memoir. As it is unnecessary for me to do more than mention this, I refer the reader to Dr Watson's admirable work. The cases will be found in Volume ii. p. 514, et seq.

The Watching Force consists of 304 men. These I formerly explained were divided into a night, a day, and a supernumerary force. The simple appellations night and day will at once suggest the nature of the duties which these men have to perform. It is not so easy, however, to determine the office of a supernumerary by the nature of the term, and I may therefore be allowed to explain it. At one time there was in connection with the Police Establishment a large body of men who went under this designation, but whose duties differed materially from those now belonging to that class. The greater proportion of these were men who followed other employments, and were only summoned to police duty in riots and other emergencies. Some occasionally supplied the places of men confined by sickness, but a great part of them never were, nor never were intended to be, incorporated with the force. No such class is now in existence. Those who go under the name at the present time are almost constantly on duty, and are necessarily as much a part of the establishment as either the night or the day men. They amount to 24 in number; and if not all required to fill vacant stations, they are disposed of in supplementing the force in those parts of the city where their services are most wanted. This body consists of the men who have most recently joined the establishment, and may not improperly be termed recruits. The period during which they serve in this capacity varies from two to four months, and they are advanced into the force in regular succession as vacancies occur. The fact of these men being at once called into active service, and subjected to fatigue and exposures to which they were formerly unaccustomed, will go far to explain any difference in the amount of sickness which exists betwixt them and those inured to police duty. There is, however, another circumstance which it may be proper in this place to advert to, viz., the fact that they are not so well clothed as the other men in the force. After having worn the police uniform for a period of twelve months, the men surrender it to the commissioners of police; on which the dress, after being repaired, is given out for the equipment of the supernumeraries. In what proportion each of these causes contributes to swell the sick list of the supernumerary force, it is impossible accurately to determine; but there can be little doubt, that, individually or combined, they are, if not the chief agents, certainly of considerable influence, in operation.

The amount of sickness amongst the force during 1846 exceeds that of the previous year by nearly 33 per cent.; an increase which will be shown in the sequel not to be proportionally greater than that to which other classes of society have been subjected, particularly the members of the yearly benefit societies. This increase, however, has not been equal in all the three classes. During 1845, there was a difference of 90 per cent. between the sickness of the night and the day force, whilst in 1846 the difference only amounted to 47 per cent. The contrast between the regularly-established night force and the supernumeraries is, however, reversed. The latter had at the rate of 185 per cent. more sickness than the former in 1845, and in 1846 it has increased to 208. The greatest proportional increase for the last year has been in the day force, in which it amounts to no less than 61½ per cent., whilst the night force has only increased 18½, and the supernumerary force 37 per cent. But in order to render this more clear, I introduce the following table in illustration.

TABLE I.—*Showing a Monthly Return of the Sick in the Night, Day, and Supernumerary Forces.*

	Sick in Night Force	Sick in Day Force	Sick in Supernumer. Force.	Ratio p. cent. in Night Force.	Ratio p. cent. in Day Force.	Ratio p. cent in Supernum. Force.	Total Numb. Sick per Month.	Average Ratio p. cent. in Whole Force.
Jan.	34	15	12	17.97	16.48	50.00	61	20.06
Feb.	48	23	8	25.39	25.27	33.33	79	25.98
Mar.	54	16	14	28.57	17.58	58.33	84	27.63
April,	54	22	13	28.57	24.17	54.16	89	29.21
May,	49	10	6	25.92	10.98	25.00	65	21.38
June,	25	14	11	13.22	15.38	45.83	50	16.44
July,	46	20	3	24.33	21.97	12.50	69	22.69
Aug.	30	20	6	15.87	21.97	25.00	56	18.42
Sept.	59	11	8	31.21	12.08	33.33	78	25.65
Oct.	48	16	17	25.39	17.58	70.83	81	21.64
Nov.	40	12	8	21.16	13.18	33.33	60	19.73
Dec.	41	32	10	21.69	35.16	45.83	83	27.30
	528	211	116	279.29	231.80	487.47	855	281.25

There are 189 men on night duty, 91 on day duty, and, as already stated, 24 belonging to the supernumerary force, making in all 304. This table shows that there have been 528 cases of sickness in the night force, being at the rate of 279 per cent.; in the day force 211, or at the rate of 231 per cent; and in the supernumerary force 116, being in the ratio of 487 per cent.; making together, 855 cases, or an average ratio of 281 per cent. on the whole force.

Besides establishing these facts, the table furnishes many others equally important, and demands from us a careful and searching analysis. In the monthly returns contained in the first column are presented one or two facts which cannot elude our observation. Amongst these, the comparatively small number of sick in the night force in the month of January is not the least important. There are only other two months in the year in which the number has been so low, viz. June and August. If the number is compared with that for the same month for the preceding year, it will be found to be little more than the one-half,—that of 1845 being 62, whilst that of last year was only 34. On the other hand, we find that the sickness in March, April, September, and October of 1846, presents a remarkable contrast to that of the corresponding months of the preceding year, that of September especially being much more than doubled, the number in the one being 25, and in the other no less than 59. February, June, July, August, and November last year are nearly the same as in 1845. In May and December there was a very considerable decrease in the number of cases in 1846.

In the day force, arranged in the second column of the table, and in which the greatest increase of sickness has taken place during the past season, there are observed many differences still more striking and important. An increase has taken place in nine months out of the twelve. In some of them it has been in the ratio of 100 per cent., and in one of 200 per cent. The only month in which the decrease is remarkable is November, the number being, in 1845, 21, and in 1846, 12 cases.

The only thing deserving of remark in the third column, is that in January 1845 there were only two supernumeraries sick, whilst in 1846 there were twelve. During the latter year there was also a considerable increase in October.

Taking the total number of sick as represented in the seventh column of the table, we find that there are three months of last year during which the sickness was considerably less than in the preceding one, viz. January, June, and November. June in 1846 was the healthiest month of the year, and, with the exception of January, which gives the same number, the most unhealthy in 1845. It will be our business by and by to inquire into the cause of this difference, but in the meantime we shall proceed to take a review of the influence of districts upon the health of the night force; and I introduce the following table, which shows the monthly number of sick in that body, and the section to which they belong.

Table II.—*Showing the monthly number of sick in the Night Force for* 1846, *and the section to which the men belong.*

No. of section.	1	2	3	4	5	6	7	8	9	10	11	12	Monthly No. for the whole year.
January,	4	2	1	2	4	4	3	3	3	3	4	1	34
February,	11	0	3	2	2	5	5	2	3	6	4	5	48
March,	3	4	5	4	1	6	2	6	4	9	2	8	54
April,	6	2	3	8	5	2	4	4	8	4	3	5	54
May,	7	7	3	3	4	4	5	1	2	4	4	5	49
June,	4	4	0	4	1	3	3	1	0	3	1	1	25
July,	4	3	1	5	3	8	7	1	5	4	4	1	46
August,	3	5	0	2	3	5	3	2	2	2	2	1	30
September,	6	2	2	4	5	6	9	4	5	10	2	4	59
October,	3	3	2	5	6	10	4	1	6	6	2	0	48
November,	6	2	1	4	3	5	4	0	3	5	3	4	40
December,	7	3	3	5	5	4	4	1	4	1	3	1	41
No. in each Sect. for the whole year,	64	37	21	48	42	62	53	26	45	57	34	36	528

In a similar table brought forward in my report last year it was shown that there was a very remarkable difference in the amount of sickness in different sections. In one, for example, it was shown that it was as high as 393 per cent., and in another 168, making a difference of not less than 225 per cent. The possibility of a slight difference was quite conceivable; but few were prepared for so striking a contrast. It was so great indeed, that a greater could scarcely be anticipated. It is, however, exceeded during the last year by nearly 100 per cent. The greatest amount of sickness in 1845 was in the 4th, and during the last year in the 6th section. The sanatory condition of these two districts is so similar, that a very slight alteration of circumstances—of wind or weather—will throw the balance in favour of the one or the other. This table shows that a decrease has taken place in five of the sections during the last year; but with the exception of that in the 9th—which, it will be remembered, was amongst the most unhealthy the previous year—the difference is scarcely deserving of observation. In the 8th there is only an increase of one, and this was amongst the healthiest districts in the town during the last year, and was the healthiest but two in 1845. The two which then surpassed it were, the one in which Stockbridge is situated, and the other, of which Newington forms a part. Both these still hold a very favourable position in the table. As the accumulation of facts in this manner may ultimately determine the sanatory character of every district in Edinburgh, it may, perhaps, be well to describe the boundaries of the sections referred to more carefully than I thought it on a former occasion necessary to do.

Section 1st, Is included within a line commencing at the head of the Canongate, extending down St Mary's Wynd, up the north side of Cowgate to foot of Blair Street, thence along east side of High Street including the closes, along by Victoria Terrace to the Castle gate, from which it is again reflected, and proceeds by Ramsay Gardens to the Royal Institution, and thence by railway station and Old Physic Gardens, up Chalmers' Close, and down the High Street to the head of Canongate, from which it started.

Section 2d, Is included within a line commencing at the head of the Canongate, extending northwards by Leith Wynd, north side of Jail and Calton Hill by Royal Terrace to the head of Easter Road, and from that to the Watering-stone at Meadow Bank, and thence by the Duke's Walk and Abbey, up the east side of Canongate, including the closes to head of St John Street; and then to South Back of Canongate up by St Mary's Wynd to head of Canongate.

Section 3d, Is included within a line commencing at the head of Infirmary Street, extending along Nicolson Street by Easter Crosscauseway, down Pleasance, by Carnegie Street to Dumbie Dykes to St Ann's Yards, up by the South Back of the Canongate and Cowgate to the foot of Robertson's Close, and from that to Infirmary Street.

Section 4th, Commences at the foot of Niddry Street, and includes both sides of Cowgate and Closes to George the IV. Bridge, and south side of Cowgate, to the foot of Candlemaker Row; from thence the boundary extends along the Forrest Road, Meadow Walk, to Fingal Place; from thence round the eastern division of the Meadows back to the west end of Buccleuch Place; is then reflected eastwards by the north side of Buccleuch Place, and through Buccleuch Pend to Nicolson Street, from which it extends forward to South Bridge.

Section 5th, Commences at Fingal Place, runs along the Lovers' Loan to Grange House; then runs westward to Canaan Park; runs south to the Jordan Burn, from which it follows the course of the Pow Burn by Black House to Mayfield Loan Toll, from there to Echo Bank on the Dalkeith Road to the east of the Dalkeith Railway Station, and Queen's Park to the foot of Carnegie Street; and thence by Pleasance, St Patrick's Square, south side of Buccleuch Place, round the eastern extremity of the Meadows, and then west, to Fingal Place.

Section 6th, Commences at George the IV.'s Bridge, and extends along by the new west approach on the south side of the Castle and Castle Terrace to Lothian Road, along which it extends by Downie Street and Wellington Street to Toll Cross by the east end of Home Street to the Meadows; thence by Meadow Place and Middle Meadow Walk, Candlemaker Row, down north side of Cowgate to George the IV.'s Bridge, the point from which it started.

Section 7th, Commences at Fingal Place, extends by Lovers' Loan to Grange, thence west to Canaan Park, from which it runs south to Jordan Burn, and from that west to the Lunatic Asylum at Morningside. It then runs in a straight line by Merchiston Bank to North Merchiston, to the Slateford Road, from which it runs south by Fountainbridge; includes the Canal Basin, west by Wellington Street, south side of Meadows to Fingal Place.

Section 8th, Is bounded by a line commencing at the head of Queensferry Street, running out the Queensferry Road to beyond the Dean Church; thence in a straight line to Dalry, and from that to the Slateford Road at Branfield Place, and from that by the east side of the Canal Basin to the corner of St Anthony Place by Lothian Road to the head of Queensferry Street.

Section 9th, Commences at the head of Hanover Street and extends to Great King Street, from which it extends westward by the upper part of the Royal Circus, Gloucester Place, Moray Place, Stewart Street, Ainslie Place, Randolph Crescent, to the Dean Bridge; from thence to the west end of Princes Street by Queensferry Street and east Princes Street to the Royal Institution.

Section 10th, Commences at the centre of the North Bridge, runs east by the Physic Gardens and west side of Jail by Calton Hill to Catherine Street, down Broughton Street by London Street, Drummond Place, and Great King Street, to foot of Dundas Street, and from that south to Princes Street, thence east Princes Street by New Buildings to centre of North Bridge.

Section 11th, Commences at Greenside Street, extends by Greenside Row, Greenside Church, Leopold Place, from which to the head of Easter Road and Janefield Place, Duke Street, to the foot of Leith Walk. From the foot of Leith Walk it extends to the Leith Distillery, follows the course of the Water of Leith as far as the Edinburgh and Newhaven Railway, by which it is bounded to Canonmills, from which it proceeds by Bellevue Crescent and Broughton Street to head of Greenside Street.

Section 12th, Commences at the east end of London Street, proceeds to Canonmills, follows the line of the Edinburgh and

Newhaven Railway to the Ferry Road; thence west to the head of Granton Road, from which it extends by the west side of Comely Bank to the Queensferry Road, and thence by the Dean Bridge, Moray Place, Doune Terrace, Royal Circus, Great King Street, and Drummond Place to London Street.

In order to condense the facts which we find detailed in the last table, and bring the ratio of sickness at once before the mind, I introduce another table, in which also the sickness of each section in 1845 will be contrasted with that of 1846.

TABLE III.—*Showing the number of men sick in each section, and the ratio per cent. of sickness.*

No. of section.	No. of men in section.	No. sick in each section.	Ratio per cent. of sickness.	No. sick in same sections in 1845.	Ratio per cent. in same sections in 1845.
No. 1	19	64	336.84	53	278.94
2	14	37	264.28	41	292.85
3	11	24	218.18	29	263.63
4	15	48	320.	59	393.33
5	17	42	247.06	33	194.11
6	12	62	516.66	40	333.33
7	12	53	441.66	35	291.75
8	13	26	200.	25	192.30
9	20	45	225.	61	305.
10	20	57	285.	50	250.
11	15	34	226.66	40	266.66
12	16	36	225.	27	168.75

In this table it will be observed that there is a difference between 1845 and 1846, in the first section, of very nearly 58 per cent., being more than a sixth part in favour of the former year. In the fifth section, the difference is about 53 per cent., or within a fraction of a fourth part more. It is still more in the sixth section, amounting to 183⅓ per cent., being more than a third of an increase in 1846. In the seventh section, the difference is not quite so great as in the latter, but still very high, being 150⅓ per cent. There is a difference in several of the other sections also, but it is not so great as to call for particular observation.

Having thus briefly noticed the amount of sickness in the police force and in the sections, and compared it with that of the preceding year, I shall in the next place endeavour to inquire into the *causes* of it. This is a task of a much more difficult nature, but is one which, on that very account, ought to be attempted. Nor are we altogether without encouragement to persevere, or grounds on which to proceed in the inquiry.

The numbers presented by the last table furnish one or two facts which serve as a key for the solution of the whole matter. I would ask any one to look at the first, fourth, sixth, and seventh sections, and say if the number of cases of sickness is not an accurate index of the accumulated filth and fetid impurities and emanations which exist within the bounds of these districts? Winds, rain, and temperature, may each or all have a temporary influence in swelling the sick list; but no cause is so constantly in operation, or produces so certain, so powerful, and deadly an impression, as the malaria arising from imperfect drainage and defective performance of scavenger duty. Both of these causes are known to exist to a considerable extent in all the sections referred to; and in those, where the sickness is much less, it is equally well known that both have been much more attended to. There is one district, viz. the seventh, the circumstances of which at first sight appear to militate against this theory. It is hard to believe that Morningside, the Montpellier of Scotland, is, in point of sickness, on a par with the Cowgate and Grassmarket of Edinburgh. Such a supposition, fortunately, does not require to be resorted to in order to explain the facts. In that district bordering on the canal, far from Morningside, is a focus of malaria, which emits poison night and day upon the unconscious inhabitants, and silently spreads sickness, death, and devastation around. I refer to Thorneybank, a place teeming with filth of every description. The blood and animal refuse from shambles, the dung from piggeries and byres, are mixed together, and swelled by the leaven of undue detention into enormous heaps. To this may be added the unfortunate position of the locality. All the places in the neighbourhood are considerably below the level of the canal which surrounds them, and have no declivity by which the drains can be cleared, and they are always covered with surface water. The sickness in this district has no doubt increased amongst the police during the last year, as it has done amongst all others similarly situated, by the higher temperature producing a greater evaporation than that which took place the previous year.

Whilst adverting to districts in which there has been more sickness during the last than in the preceding year, the fact that there is one, in which there has been no less than 80 per cent. less, must not be forgotten to be stated. This section is so favourably situated, that in no locality in Edinburgh could less sickness have been anticipated. Yet, in 1845, it was amongst the worst. The ratio of sickness then was 305 per cent., the greater part of it being during the winter and spring months; the last year it was only 225 per cent. It was attempted in my last report to explain how the great amount of sickness had been produced in so healthy a locality, by supposing that it was on account of its exposure to the north winds. This view was so far strengthened by the fact of most sickness having occurred during the winter months. This is still further confirmed by the past year's experience, for none of the severe colds which were felt during 1846, were accompanied by north winds. And again, during January and February of the current year, when the frosts and colds were very severe, and accompanied for the most part by a northern breeze, the sickness in the ninth section greatly increased.

There is another circumstance affecting the health of the watching force of the police establishment, to which I must advert before concluding this part of my report. I refer to a large number of the men living together in one house, called a Section House. I illustrated this rather fully on a former occasion, and had imagined that, in consequence of improvements which were made immediately thereafter by the commissioners of police, no further remarks would have been required upon the subject. The result has turned out contrary to anticipation, and a love of truth compels me, contrary to my inclination, once more to demonstrate the real state of matters by the power of figures.

TABLE IV.—*Showing the difference betwixt the health of the men in the Section House, and those residing out of it.*

Total number sick in the whole force,	855
Number *out* of Section House, . .	717
Number *in* Section House, . .	138
Ratio per cent of those *out*, . .	268.53
Ratio per cent of those *in*, . .	372.97
Difference in favour of those *out*, .	104.44
Ratio per cent in Section House in 1845,	351.35
Difference in favour of 1845, . .	21.62 p. cent.

In this table we have indubitable evidence that this house is injurious to the health of the men who reside in it; sickness is at the rate of 104½ per cent. greater amongst them than amongst those residing out of it. This difference is altogether inexplicable on any other supposition than this, viz.—that they are exposed to some noxious influence to which the men residing elsewhere are not subjected; and instead of being ameliorated by the alterations which have been made, we find that the ratio of sickness during the last year is 21½ per cent. more than it was formerly.

There can be no doubt that the main cause of the sickness in the section-house, is the want of proper ventilation. The men who reside there are in most respects better accommodated and under better regulations than those residing elsewhere. It is a fact corroborative of this view, that there are a few lodging-houses in town principally frequented by policemen, where bad ventilation is very obvious, and where three and four individuals sleep in the same small apartment, in which the men are similarly affected.

It is unnecessary to give a tabular view of the diseases which have occurred during the past year. Amongst these, affections of the respiratory organs have greatly predominated; next, diseases of the stomach and bowels have been most frequent. From about the 12th of August to the end of October, diarrhœa was of very common occurrence, and amounted to about 30 per cent. of the whole cases on the sick list. In June, there was, in my opinion, an undoubted case of *cholera Asiatica.*

I am the more particular in mentioning this case, as it has been denied authoritatively that such a disease had existed.

In evidence of this being a case of *cholera Asiatica*, I may mention that there was a combination of all the symptoms which usually form that disease;—there were vomiting, purging, blueness, coldness, pulselessness, the whispering voice, the sunk eye, and peculiar cadaveric appearance of the countenance, spasms, and the rice-water dejections. It is true the patient did not die; if he had died, the nature of the disease would never have been questioned, and it would have been stamped in the records of the burying-ground, and blazoned forth in the monthly tables of mortality, as a real case of cholera. I may mention, that I am no stranger to the appearance of that disease. During the epidemic which traversed this country fifteen years ago, I saw several hundred cases, and would have no more doubt of a case now when it did appear, than I would about a case of small-pox or scarlatina. Unfortunately, the poor man had some practical experience himself in the matter, as he made a very narrow escape with his life from an attack which he had, when it raged here as an epidemic, and knew at once that it was a return of the same disease.

Rheumatic affections have been still more common than in the preceding year. The cases of fever have not been so numerous, and amongst these there have been no deaths. Only two cases of disease proved fatal during the last year, the one from erysipelas of the head and face, and another from chronic disease of the lungs and digestive organs, accompanied latterly by rheumatism. I am glad to state that sickness arising from an improper indulgence in intoxicating liquors has considerably diminished, and I can say the same in regard to the existence of venereal affections. Whether these circumstances can be imputed to the progress of morality amongst the force is not easily determined, and we shall wait the result of another year before a more positive opinion is expressed upon the subject.

It remains for me now, before concluding this report, to examine for a moment into the health of some other classes of the community, and with it to compare that of the police. I select first, as I did last year, the members of the yearly benefit societies. In these there must be a fair representation of the health of the operative classes. The number which I have collected this year is not so great as last, but sufficient nevertheless to give an average which will suit our present purpose. I have selected twelve of the largest, embracing 2869 members. The number belonging to each, the amount of sickness, &c. will be best understood by the following table:—

TABLE V.—*Showing the amount, percentage, and duration of sickness amongst* 2869 *members of male and female yearly societies in Edinburgh in* 1846.

Name of Society.	No. of Mem.	No. of Mem. sick.	Total No. of days' sick.	Aver. No. of days' sick.	Ratio p. cent. of sickness.	No. of deaths.
Mr Brown, N. B. Canongate,	648	153	5551	8.56	23.61	14
Mrs Paterson, St Leonard Street,	586	148	3366	5.74	25.25	8
Mr Tait's, High Street,	875	79	2477	6.60	21.06	6
Mr Neilson, Macdowall Street,	197	37	952	4.83	18.78	7
Mr M'Fie, Cowgate,	200	53	1351	6.75	26.50	4
Mr Somerville, Canongate,	132	46	1693	12.82	34.84	3
Mr Mushet, Carnegie Street,	162	56	1604	9.90	32.09	7
Mr Watt, St Leonard Street,	116	27	453	3.90	23.27	1
Mr M'Donald, Canongate,	92	25	621	6.75	26.08	2
Mr Forbes, High Street,	71	14	430	6.05	19.71	...
Mr Cameron, Canongate,	105	19	399	3.80	18.09	2
Mr M'Lean, Salisbury Street,	185	38	1135	6.13	20.54	4
	2869	695	20,032	6.81	24.15	58

Average per cent. of deaths, 2.02.

Out of the 2869 members, 695 have been sick during the last year, and the average number of days' sickness to the whole is rather more than 6¾ days, being nearly half a day more than it was in 1845. The fifth column in the table shows that the ratio of sickness has been rather more than 24 per cent., being about 3½ per cent. higher than the former year. The deaths, too, have been at the rate of 2 per cent. during the last year, being more than a half per cent. in favour of the year which preceded it.

From these facts it will be obvious, that there is not such a very great increase of sickness in the police force as compared with other classes of society. The proportionate increase is not so great as it is amongst the members of the yearly societies. It amounts in the latter to nearly a sixth part, whilst the difference amongst the former is only about an eighth part. The mortality, on the other hand, is not half so high amongst the police as amongst the members of these societies.

The difference between the amount of sickness in the watching force and the men belonging to the cleaning and lighting departments of the police establishment is also deserving of a moment's consideration, and is exhibited by the following table :—

TABLE VI. *Showing the difference between the amount of sickness in the Watching Force and the men belonging to the Cleaning and Lighting Department.*

Total number of sick in the watching and lighting force,	70
... of days' sickness, . .	658
Ratio per cent. of sickness, . . .	44.58
Average number of days' sickness, . . .	4.19
Difference per cent. between watching and cleaning, .	236.71
... in the average number of days, . .	7.31

The average number of men in the scavengering and lighting department is 157. During the winter months it is much higher, and during the summer is reduced to about 130. With the dirtiest occupation, and most filthy, damp, and worst ventilated houses in the city, they are nevertheless in a much more favourable position in point of health than the men of the watching force. The ratio of sickness is 2 per cent. less than it was during the preceding year, being the only body to which allusion has been made in which it is diminished. There is also a difference in their favour on the average duration of sickness, amounting to 2½ days,—the number in 1845 being 6.80, and in 1846, 4.19. As compared with the police we find that there is a difference of no less than 236.67 per cent. in the amount of sickness, and 7½ days on the average number of days. And, as compared with the yearly societies, the ratio of sickness amongst the scavengers is about 20 per cent. higher, but in the average duration of sickness is 2½ days less. How the cleaning and lighting force should be in this favourable position is a question which is not so easily explained, and which I shall leave for the discussion of the Society.

I shall now conclude these observations by proposing the two following questions, viz., Is it possible, keeping in view the nature of the duty which they have to perform, to diminish the amount of sickness in the police force? And, secondly, What means hold out the most reasonable prospect of accomplishing it?

It would be unfortunate were we compelled to answer the first of these inquiries in the negative; for a continuation of the present state of matters is anything but desirable. But as there is, besides mere night exposure, some cause or causes in operation prejudicial to the health of the police, and that these are in a less active state, or perhaps altogether absent in some districts of the town, it is very probable that, so soon as they are discovered and rightly understood, the application of a remedy will both be easy and followed by the happiest results. Were all the districts brought to a par with the eighth, there would be a very considerable diminution of sickness, and this much at least might be considered possible. Admitting this possibility, and taking the eighth section as a model, then the question occurs, What is required to put the others in the same condition? Sanatory improvement must be the general answer. It was shown in my former report, and might have been more particularly adverted to in the present, that the Cowgate and the Grassmarket were healthier than some of the New Town sections in the winter months, being better protected. Now, were draining and cleaning carried to a proper extent, and conducted in an efficient manner, I have no doubt that an increase of sickness in the summer would never take place in these districts, and that it would even diminish in the winter season. Besides the adoption of sanatory measures, something else is still required; for the sickness even in the eighth section is far too high, and the men stationed there must no doubt be envious of a scavenger's occupation, or at all events of the immunity of disease which he experiences. It is not improbable that the dull, slow, sleepy movements of a police constable is the principal cause of all his suffering. And might not some remedy be adopted for this? I would answer, increase his speed and improve his clothing. I pointed out in my last report how the latter could be done, and I hope I will not be charged with encroaching on the province of a brother officer, if I should explain how the former can be accomplished.

Instead of having one night shift, I would have two of five hours each, and convert two beats into one, and make the officer go round the whole of the increased bounds in the same space of time as he formerly took to the half. Nothing would be lost by this change, the beat would be as well covered, property be as well protected, the man kept from sleep-walking and in a comfortable state of warmth, and leave his duty much less fatigued than he does at present. This could be done, too, without walking more than two miles an hour. There are objections to this plan in the crowded thoroughfares of the city, where a large available force must be at all times at command to disperse mobs, &c. when they occur, but in all the other districts where the duty of a policeman is almost solely confined to the protection of property, no such objection obtains. The means to be adopted for improving the health of the men in the section-house are so obvious, that it is almost unnecessary to advert to them. The ventilation of the apartments must be improved in the usual manner; and if the atmosphere is still impure, the number of men in each apartment must be diminished,—by the adoption of which means I hope to appear before you next year with a more favourable report.

TABLE *showing the temperature and state of the Rain Guage for* 1845 *and* 1846.

1845. Months.	Thermometer.				Rain in inches.	1846. Months.	Thermometer.				Rain in inch.
		Min.	Mean.	Range.				Min.	Mean.	Ran.	
Jan.	53°	5°	36.16	48°	1.77	Jan.					
Feb.	49	18	34.68	31	1.67	Feb.					
March,	57	16	36.25	41	0.40	March,					
April,	64	28	44.73	36	2.24	April,					
May,	69	33	49.22	36	3.08	May,					
June,	79	40	56.95	39	1.72	June,	84	41	63.45	43	3.59
July,	70	38	54.72	32	3.48	July,	79	46	59.25	33	4.17
August,	77	40	56.79	37		August,	76	42	59.58	34	5.01
Sept.	72	31	54.21	41	1.77	Sept.	78	39	59.30	39	3.35
October,	71	30	49.21	41	6.14	October,	64	30	48.24	34	3.60
Nov.	61	28	42.76	33	1.70	Nov.	59	22	44.53	37	1.74
Dec.	55	25	38.89	30	2.04	Dec.					
Year,	79	5	46°.21	64°	26.65						
Mean of Months,	65	28	46°.21	36	2.22						

ART. III.—*On the Oriental Plague.* By HENRY M'CORMAC, M. D.

THERE is perhaps no single malady which has been subjected to greater mystification than the oriental plague. On the one hand, human dread and credulity have invested it with exaggerated communicability; on the other, it is boldly denied to be communicable in any wise. Let contagion pass. I quite agree with Mr Laidlaw, in the October number of your excellent Journal, that plague, ordinarily speaking, is uncontagious. He admits that it is infectious; and most observing practitioners agree with him.

In my humble opinion, the whole doctrine of plague, so far as it lies open to observation, may be brought within a very narrow compass. What the conditions of its existence, in my estimation, really are, I shall here, therefore, very succinctly, proceed to state.

Imprimis, I. As to the sources of plague, irrespective of infection, we are in the same state of ignorance as obtains with regard to small-pox, measles, scarlet fever. Plague, as Pariset supposes, may ensue from the offensive emanations arising from decaying animal remains; or with many others, we may derive its existence from some unknown epidemic condition of the atmosphere,—a notion which, for my own part, however, I am forced to reject. But in any case, it is found by experience that some persons, from whatever cause, remain wholly insusceptible.

II. It seems proven that either very cold weather or very warm—say below 50° F. or above 80° F.—puts a stop to, renders much milder, or greatly impedes the propagation of plague.

III. The clothes or other effects of persons sick or deceased of plague, likewise all goods and merchandize, are, conditionally or inconditionally, incapable of propagating the malady.

IV. Persons ill housed, ill clothed, ill fed, ill cleansed, and unprovided with fresh air, are greatly more liable to plague than persons in easy circumstances, who live in well-ventilated cleanly apartments; and more especially those, otherwise fortunately circumstanced, who practise isolation. It was observed that very few of the nobles, during the plague of Moscow, or their immediate dependents, incurred the disease; while, on the other hand, the great mass of the people were decimated.

V. Plague is only communicable by actual inspiration of the aerial products emanating from persons labouring under the malady, and inspired in their immediate vicinity.

VI. With ventilation, which may be termed air-cleansing, and strict cleanliness otherwise, plague ceases to be highly communicable; on the contrary, it becomes communicable with difficulty, or not at all.

VII. The incommunicability, partial or complete, under any of the foregoing circumstances, as the limitation of the progress of the disease by heat or cold, by cleanliness and ventilation, by isolation, by natural inaptitude, by the multitudes of susceptible persons being weeded out by the disorder, and by its incommunicability from mere contact, direct or indirect, are not to be held as evidence of its unconditional uninfectiousness; seeing that the infections or communicable nature of plague is proved by multitudinous testimony.

The data or postulates here ventured on I might support by very numerous authorities. If these data should prove correct, the effect would be greatly, and very beneficially, to modify our present quarantine restrictions; but they would not justify a total abandonment of these restrictions, which, however much abused, have, in my opinion, served to protect England, mainly Europe, for so long a period from a terrible scourge. It is not necessary to prohibit merchandize or goods of any description imported from a plague-stricken country. It is only necessary, in order to be quite safe, to prevent the introduction of a person actually infected with, or labouring under, the disease. The period of incubation is rarely more than two or three days, and never more than six. It is refreshing to find an intelligent and observant practitioner like Mr Laidlaw declare, that, with proper professional attendance and good nursing, a very large proportion of plague patients recover; moreover, that in private houses, where ventilation is good, it is a general rule that the disorder is confined to one person. On the other hand, he states that when ventilation, as too often happens, is neglected, every one is cut off. It is quite evident, from modern experience, that plague, like other monstrous epidemic and endemic diseases, entirely owes its existence to violated hygienic conditions; and that its dissemination would be impossible in an intelligent and comfortable community. Unfortunately, however, universal comfort and intelligence are a consummation which no nation in the world has yet arrived at. The material condition of vast masses of our own poor is no better than in the days when plague was periodically rife with us; and I much fear, unless we hasten to improve, that this dreaded malady may yet show itself among us.

Art. IV.—*Physiologico-Pathological Researches on Tuberculosis.** By Dr H. Lebert, Practical Physician in Bex, Switzerland. (Müller's Archiv. fur Anatomie, Physiologie und Wissenschaftliche Medicin. Jahrgang, 1844. Berlin, Seite 190.) From the German.

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* This term, *Tuberculosis*, has been employed by Müller, Rokitansky, Vogel, Gluge, and other writers, mostly of the German schools, in order to designate the presence of that morbid process which consists in the formation of tubercles in various textures and organs of the animal body. The term has been formed by no means according to the classical rules of etymology. The word is derived from a Latin diminutive by adding a Greek termination, and is consequently a hybrid production. In the formation of such a term, one of two courses ought to have been followed;—either to add to the Latin root the Latin termination, or, if it were deemed necessary to have a Greek termination, to annex that to a Greek root. In the first case, the ordinary term *Tuberculatio* might have answered sufficiently well. In the second, the analogous Greek term *Tyroma* would have given the word *Tyromatosis*. After giving these explanations, however, we give the epithet used by the patrons of the etymology.

[In the former part of this volume (p. 211) we stated that we should take an early opportunity of directing attention to the researches of M. Lebert, on the minute structure and formation of tubercles. We insert here the first, the most elementary and fundamental, and probably the most original portion of his inquiries. It is perhaps unnecessary, in this stage of the inquiry, to give any opinion on the actual value of these researches, further than to say that they deserve, and we hope will obtain, the careful attention of all intelligent pathological readers.

The language of the following paper, and, it may be added, the mode of thinking exhibited, partakes, in the original, of obscurities proceeding from two sources.

The first is from several of the philosophical and scholastic distinctions of German writers generally. The second proceeds from the notion entertained by the author, that it is possible to distinguish, in all examples of morbid processes, the physiologico-pathological cause of these processes, and to elucidate its nature and mode of operation, either by reasoning, or by attending to the phenomena furnished by microscopical investigation, and reasoning on them. The difficulty arising from the first can be overcome only by frequent perusal of the writings themselves; on the second of these sources of obscurity it may be proper to make a few observations.

There is little doubt that, speaking abstractly and philosophically, the principle above enunciated is correct. It is so, however, only as a philosophical abstraction; and when we come to apply it in practice, we find that it falls greatly short of the original. It does not, indeed, in all cases, admit of demonstration, or any sort of positive proof. In tracing the course of morbid processes, we are compelled, by the nature of the subject, to commence with the effects produced. We cannot perceive the primordial beginnings of inflammation and its products, of tubercle, of skirrhus, of enkephaloma, or of the deposits of fatty matter taking place in the different textures. Their effects, after they have been established, and have been present for some time, are all that we witness,—all that fall under sensible observation. The problem is, from these effects to discover the cause on which their formation depends, and the mode or modes in which this cause operates. Of this we obtain, even with the microscope, no more than glimpses, sometimes very imperfect and obscure, and sometimes not closely connected. These are to be connected sometimes by reasoning, sometimes by conjecture, or even by fancy. Facts furnished by chemical analysis and the observation of chemical changes are added, and often rendered available. And in this manner, and with these and similar means, it is attempted to discover and form a just idea of that essence or principle, which is at work in the human body in the production of diseases, whether dynamic or organic.

This distinction between the pathological cause and the effects produced, whether dynamic or organic, is not new. All intelligent physicians in this country have been long aware, that, though it is important and necessary to know the effects of disease, and the various organic changes which are liable to attack different textures and organs, yet these are no more than effects, and that it is still necessary to look a step higher in the concatenation, in order, if possible, to discover the cause or causes. In studying, however, to find this more essential agent, they have hitherto encountered great difficulties and impediments,—difficulties which are perhaps insuperable, and impediments which cannot be removed.

The great difficulty appears to consist in this circumstance, that, though there may exist in the animal body, both in its fluid and solid textures, some general essence which causes all morbid actions and their effects, yet it has hitherto been impossible to demonstrate the existence of this essential agent in any other manner than by its effects.

Another circumstance which, in any inquiry of this kind, demands particular attention, is, that disease, whether defined as an action or process, or the effect of an action, is, properly speaking, the negation or absence of health—the deviation, more or less complete, from the normal physiological state. In strict language and correct thought, therefore, disease cannot be said to consist in anything positive. It is a negative state, that is, a state more or less opposed to, more or less departing from, the state of health.

It is highly necessary, in studying the observations of such writers as M. Lebert, to keep this steadily in view. With them, as health is a physiological process, so disease is a pathological one, yet always bearing such relation to the physiological or normal state, that the latter may be traced from the former, and even the healthy phenomena may be recognized in their transition to the morbid process. Hence the use of the term physiologico-pathological, and the frequent reference to what he terms the physiology of morbid phenomena. It is not difficult to see, that by this expression, which to some may seem to imply a contradiction in terms, he means simply the observation and study of the vital process which causes morbid phenomena. In truth, as there is a physiology of health, so there is a physiology of disease, that is, a vital process going on in the living animal body, giving rise to various morbid effects.

We take it for granted that no one denies that there is in disease a vital, or, it may be, an animal process, as much as there is in health; that is to say, a process consisting of peculiar changes, and usually attended with formations characteristic of morbid action, and to which changes the name of vital or animal may be given, solely because they do not resemble physical changes. It may turn out, that these changes are similar to, or the same with, physical changes. But it does no harm at present to consider them as distinct. In many, the most remarkable feature of these changes is, that, whereas in health, chemical actions appear to be subordinate to them; in disease, chemical actions get the better of them.

Admitting, therefore, the existence of this process, vital or animal, or both, the object of pathology is to ascertain its nature and its cause or causes, and to discover, if possible, its mode of operation; and it may be added, that the great practical reason for correctly understanding these points, is to know if we possess means which might be employed early and in proper season for arresting the progress of the morbid action, and thereby curing the disease, and restoring the patient to health.

Now, in this inquiry, M. Lebert appears to presume, that if we know the cause of the disease and the mode of operation, we may know also, or we may discover, the means of preventing its development and stopping its progress. This conclusion is not by any means well established. For experience shows that we may know causes and understand their mode of operation with considerable accuracy, yet possess no means of certainly checking that operation.

On the other hand, it must be allowed, that however carefully and diligently pathologists have sought to discover true causes of disease, that is, agents which when present produce disease, and on the withdrawal of which, disease ceases to be produced, their efforts have hitherto been productive of little advantage. And in no department of pathological inquiry has this been so strongly illustrated as in that relating to the formation of tubercles. Omitting the early researches of William Stark, which, though most elaborate and accurate, were still too much of the nature of mere morbid anatomy descriptions, and, though giving correct delineations of the morbid changes, did not profess to ascend to the cause and agent of these organic changes,—we have had, during the last twenty-five years, the observations of Magendie, Schroeder Van der Kolk, Dr Carsewell, Ravin, Kingston, Bright, Addison, Andral, Rainey, and others less known. All these inquirers have, in describing the morbid changes, laboured more or less directly to trace these changes to their first origin and cause, and all have thrown more or less light upon the mode in which tubercles are developed, especially in the lungs. Sir John Baron also has deavoured to elucidate the formation of tubercles in general, partly by experiments on the lower animals, partly by reference to the phenomena and course of tubercular growths in the peritonæum. Yet, all that we know is, that the bodies called tubercles are formed at first by the effusion and deposition of a soft, semifluid, plastic matter within the air-cells of the lungs, and which matter is secreted by the membrane and vessels forming these air-cells. We know not, however, what causes the cellular membrane and vessels to secrete this plastic semifluid matter; in what particular state the membrane is when it secretes this semifluid matter; we know no certain and invariable signs by which the actual process of its secretion is indicated; and we cannot say that we are aware of the existence of any means by which this morbid secretion could be arrested and prevented from causing its usual hurtful effects.

Again, as to tubercles forming within the *peritonæum* and over the surface of that membrane, it is certain that their first development takes place in a very insidious manner; that the symptoms, if any, which attend their first appearance, are very variable; that sometimes diarrhœa, and sometimes constipation, takes place; that pain in some part of the abdomen, though frequent, is not an invariable symptom; that swelling takes place only after a certain time; that fluid is effused within the cavity of the *peritoneum;* but, that when these phenomena have shown themselves, the disease is well advanced, sometimes so far, that the termination in death is not remote. It is true, that in the case of all tubercular formations, the skin has a peculiarly dry, harsh feeling, does not transpire naturally, and, at certain periods of the nycthemeral revolution, it is preternaturally cold, and at others warm and perspiring; that the patient undergoes a process of rapid wasting and emaciation; that whatever food he takes, nourishes not; and that the urine is scanty, and usually deposits a sediment. But in how many instances of organic changes do we find the same state of the skin, the circulation, and the secretions? In short, few subjects in pathology are involved in greater obscurity than that of the cause and origin of tubercular deposition. Indeed, of the cause of tubercular deposition within the *peritonæum* and over its surface we know nothing. That these bodies are formed there, and that their presence is attended with accretion of the folds of the *peritonæum* and sero-albuminous effusion we see and know; but the cause of all this is completely concealed.

With regard to the statement made by M. Lebert, as to the great object of all pathological research being to connect pathological principles with therapeutic indications, this object has been steadily kept in view by many inquirers into the nature of pulmonary consumption. Thus, Schrœder, Dr Carsewell, Sir James Clark, Dr Bright, Dr Addison, and Ravin, all endeavour, in tracing the progress of tubercular deposition, to do so in reference to remedies to be employed, either prophylactic or therapeutic; and a large body of practical writers have more or less forcibly and directly appealed to the principles established by these authors, in order to confirm and enforce several of the therapeutic measures recommended by each. It cannot, therefore, be said that M. Lebert is the first who has thought of or attempted to establish this union, the propriety of which has been recognised in this country for a long series of years.

These explanations are rendered requisite by the statements made by M. Lebert. In giving them we wish to find no fault with M. Lebert, whose researches deserve the gratitude of all who take interest in the progress of pathological knowledge. We do not even wish to receive coldly the sanguine and confident anticipations in which he indulges, as to the microscopical revelations being capable of improving very much our therapeutic success in the treatment of tubercular diseases. On the contrary, we wish to give them all reasonable encouragement; and we think it fortunate that the subject has fallen into the hands of an inquirer at once skilful, spirited, and confident of success. Even if his endeavours fall short of what he himself anticipates, good cannot fail to result from them. Whatever truth and accuracy they contain will stand and increase in value. Any errors, any disappointed expectations, any deficiency of anticipated results, will only lead to more careful research and more cautious analysis and induction.

One inference results from all the observations made by Magendie, Schrœder, Carsewell, Ravin, Rainey, and, we may add, the present author, on tubercles, and it is important, and not void of interest. This is, that however great be the amount of disorganization observed in the structure of the lungs in cases of fatal phthisis, the formation of tubercles is at first manifestly the effect of a functional or dynamic disorder in the vesicular membrane. It seems singular, that when we contemplate the great and striking changes of structure observed in the lungs of persons who have been destroyed by phthisis, yet it is manifest that the state which led to and terminated in these changes was at one time a mere functional disorder of the vessels of the vesicles and their membrane. This appears from the following facts.

It results from the observation of all those who have traced and studied the early formation of tubercles, that they appear first in the form of a semifluid or fluid matter within the vesicles; that this fluid exudes as a secreted product from the membrane forming the vesicles, and from the blood-vessels numerously and minutely distributed on that membrane; that being plastic, or possessing the property of self-coagulation, this fluid coagulates and becomes solid; that in so doing, it not only fills but obliterates the vesicles in which it has been effused, and is moulded within them, adhering to their surface, so that it cannot be expelled. The vesicles of the lobule or lobules, or even part of a lobule, thus filled with plastic matter, are no longer permeable to air. They lose their natural softness, sponginess, and elasticity; and the portion of lung becomes firm, hard, and compact. When this has proceeded for some time, the same action which had commenced in the interior of these vesicles extends to their exterior, and unites them in one solid mass, and thence to the perilobular filamentous tissue, forming what then falls under observation as a tubercular mass, a hard nodulated body, roundish, spherical, or irregular.

Now, in this process it is certain, that at its commencement the vesicles contained only air, admitting that fluid as it was drawn in by the windpipe and bronchial tubes, and partially expelling it as it was withdrawn in expiration. It is also certain, that the lining membrane forming the air-cells or vesicles neither contained any fluid or semifluid or solid matter, while the air was thus filling them, and they were undergoing alternate distension and contraction. The effusion of the semifluid or fluid matter is the first indication, or rather the first effect of their assuming a new action. Everything tends to show that this new action consists in some great change in the numerous minute vessels ramified in the vesicular membrane; and consequently it is, before the secretion of the new product, a mere functional disorder.

Of the nature and cause of this action little is known; but it is conjectured that it is of the nature of inflammatory congestion; and from other collateral evidence it is believed to be under the influence of the strumous disposition.

The vesicular membrane has been shown by Mr Addison and Mr Rainey, to be different from the bronchial membrane. The latter, indeed, cannot be traced into the small terminal divisions or ampullular cavities called vesicles and air-cells; and the proper vesicular membrane appears not to be a mucous membrane, but to assume the characters and properties of a vascular serous membrane. These circumstances, if they do not explain, appear to be closely connected with, the facility, with which the pulmonic vesicular membrane secretes plastic matter and a semifluid, which contains the peculiar substance which afterwards presents the characters of tubercle. It is to be remembered, on the other hand, that even the bronchial mucous membrane secretes in certain cases of inflammation plastic matter, which is then found lining the bronchial tubes more or less completely, and more or less extensively.

Let it be here further observed, that the term tubercle is employed by many writers in two senses. In one it is applied, as it was by Morgagni, Cullen, Stark, and their contemporaries, to signify the large, hard, irregular, compact masses found in the lungs, often entirely or partially excavated, in persons cut off by consumption. In another sense it is applied to denote the small bodies formed in each vesicle, or at most, in several contiguous vesicles, by the effusion and consolidation of plastic tubercular matter. It takes many of the latter, probably hundreds, to compose the former masses. The small integral bodies are not always the same either in consistence, colour, or perhaps nature. The large masses are still less homogeneous; and they agree only in being hard, firm, and inelastic, and rendering the portion of lung which they occupy impermeable to air, and unfit for the uses of respiration.]

When we take a comparative view of the different morbid processes which among mankind cause the greatest ravages, we find none which can be compared with TUBERCULOSIS, no one which at all times, in all countries of the temperate zone, at all ages, and especially at the most hopeful period of youth, claims so many victims. It has accordingly been the purpose of physicians at all periods to direct great attention to this malady; and from ancient periods to the present day, a long series of works on this important subject of pathology from time to time has appeared.

The ancient physicians, little conversant with pathological anatomy, and mostly occupied with an empirical symptomatography, have occupied themselves principally with the therapeutic means supposed capable of curing the disease. We have, accordingly, a copious store of Materia Medica articles against phthisis; while the conception of phthisis is so indefinite, embracing in itself so many different morbid processes, that this copiousness is entirely illusory,—a result which has led many candid physicians to distrust and skepticism.

The modern schools, influenced by the conviction, that objective semiography and pathological anatomy ought to be the foundation of any rational system of therapeutics, have directed particular attention to this department of medicine. From this tendency not only has science derived advantage, but labours honourable to mankind have resulted. Yet we often painfully desiderate, in this school, the accomplishment of the most sacred duty of the physician, viz. to aim directly at the curing of those morbid processes, of which exact pathological knowledge is only an incomplete part.

The cause of this barrenness, in a therapeutic point of view, is especially to be imputed to the modern French school. On the one hand, the most eminent chiefs of these doctrines fell into the gross error, that practical medicine may and ought to be deduced entirely from scientific principles; and on the other hand, their rectification was rather an anatomical than a physiological essence, rather a topographical character than one entering deeply into the substance of the pathological phenomena.

The present time appears accordingly to occupy the interval between the ancient school, the object of which was chiefly therapeutics, and the modern school, which aimed at more rational knowledge, yet often failed to attain the object of the physician.

The only proper path to be taken, and the first step in the inquiry, appears to us to consist in exact researches on the peculiar physiology of morbid phenomena, in order to found a system of special pathological physiology resting on the basis of manifold observations. For this undertaking we already possess excellent materials; but it must also be acknowledged, that these are mere first beginnings, and no more than fragments of a more elevated medical doctrine.

Tuberculosis appears to us to belong especially to that order of subjects for which, in this point of view, hitherto little has been done, though, for the purpose of practitioners, this justly demands more accurate investigation.

Having been for several years engaged in researches in this department of the science of medicine, I shall study to present, in the following memoir, a portion of my inquiries. I must, nevertheless, beforehand solicit the kind indulgence of the reader, as I truly feel how many deficiencies these researches contain.

I shall divide this contribution, embracing the greatest portion of my own observations, into four sections. In the first, I shall particularly describe the elementary constituent parts,—the microscopical molecules of tubercle. In the second, I shall illustrate more intimately its differences in the manifold forms and organs in which it takes place. The third section shall contain, deduced from the two first, the physiology of the different phases of the tubercular process. Lastly, in the fourth section, I shall endeavour to place the general therapeutic indications in unison with those pathological inquiries, and particularly I shall labour to consider those therapeutic problems, which science has yet to solve.

SECTION I.—MICROSCOPICAL STRUCTURE OF TUBERCLES.

In observing the molecular composition of pathological formations, it appears that they are regulated by a general law, in consequence of which everything actually pathologically different presents this difference in its minute microscopical structure. This important law has hitherto not been rightly recognized, and has not been generally announced, for this reason, that in the study of tubercles, pathologico-microscopical researches have not been conducted with proper extension, perfection, and comprehensiveness; and, on the other hand, sufficiently high magnifying powers have not been used in the inquiry. With enlargement of 300 times, which most mikrographers only in exceptional cases exceed, the small elementary cells, scarcely above from one to two hundredth parts of one millimetre in size, so closely resemble one another, that it is scarcely possible to recognize with certainty their naturally determinate differences. I have made my collected observations with an Oberhäuser's microscope; and I am indebted to the kindness of this distinguished artist for the best lenses which have proceeded from his establishment.

Next to this I have always tried the different objects of my pathological researches, successively from a very low power,—for instance, 25 to 50 times,—to a magnifying power of 800 times. I have taken my measurement in diameter to the four hundredth part of a millimetre, sometimes to the eight hundredth part, and even to much greater minuteness by means of the *camera lucida.*

These explanations I consider to be not only necessary for forming a just judgment of my researches, but they are premised especially with the view of obviating the objection of not having seen in tubercles a great deal, which others have described in these bodies. I must, however, confess, that, on careful perusal of the facts hitherto made known on the minute structure of tubercles, I find much that is accidental confounded with what is essential, and much described and indicated which has no existence,—an additional reason why I should endeavour in the observation of nature alone to search for the solution of my problem and the determination of my doubts.

Above all, we must carefully distinguish the elements presented in all forms of tubercles from those which appear only in an exceptional manner, and especially from those which, being confounded with the microscopical preparation, led both ourselves, in the beginning of our observations, and many other observers into error,—to consider these exceptional and accidental phenomena, as pertaining to the essential constituent parts of tubercle.

The invariable elements of tubercle are the following:—

1. A large amount of MOLECULAR GRANULES, perfectly round, whitish-gray in colour, or passing into yellowish, sometimes compact, sometimes appearing slightly transparent in the centre, from $\frac{12}{10000}$ to $\frac{27}{10000}$ of a millimetre in diameter. These granules are found, in particularly large amount in softened tubercle; in crude yellow tubercles, they conceal, and often prevent the recognition of, the peculiar tubercular globules.

2. These granules and the peculiar CORPUSCULA, presently to be more minutely described, are closely united to each other by means of a HYALINE INTERCELLULAR SUBSTANCE of moderate consistence. This substance becomes first fluid in the softening process.

3. Though the two now mentioned constituent parts present

nothing characteristic, we have yet to notice the most important—the peculiar constituent part of tubercle. These are the proper tubercular CORPUSCULA, which I have described as globules or corpuscles peculiar to tubercle, in the last edition of the work of Louis.*

The shape of these *corpuscula* is seldom entirely round, though it is probable that, at the first separation of tubercular matter by capillary transudation, they approach the globular shape, and that they acquire an irregular figure by the narrow space in which they are deposited, and by mutual compression. But, in the circumstances in which they are usually placed under the microscope, they present manifestly an irregular figure, being sometimes spherical, sometimes approaching the oval shape. They are usually irregularly angular and polyhedral, with edges and angles most commonly rounded, as is seen very distinctly when they are allowed to float in water or serum or any other thin fluid. Their colour is clear yellowish. Their exterior is throughout irregular. We observe in them a heterogeneousness of content which gives the interior sometimes a flocculent, sometimes a cloud-like or nebulous aspect; but never did I find in them genuine *nuclei*, notwithstanding the greatest care employed for the purpose, either with the best magnifying powers or with different chemical reagents. There are, indeed, mostly dispersed in the interior substance of the tubercle irregular MOLECULAR GRANULES, scarcely exceeding $\frac{25}{10000}$ of one millimetre in size, sometimes only from $\frac{12}{10000}$ to $\frac{15}{10000}$ of a millimetre; these granules, three, five, ten, or more in number, are throughout not visible in the like layer in a determinate type. The intergranular substance in other respects invests them firmly without clear covering; the interior of the granules I never found transparent. The magnitude of the tubercular cells varies; those most spherical are from $\frac{5}{1000}$ to $\frac{75}{10000}$, sometimes to $\frac{1}{100}$ of a millimetre in diameter. The oblong tubercular cells I found on an average $\frac{75}{10000}$ of a millimetre long, and $\frac{5}{1000}$ to $\frac{6}{1000}$ broad.

We shall afterwards see that the diameter of these globules increases in the process of softening. I have, in all my observations on tubercles, taken micrometrical measurements, and some of these are here subjoined.

	Millimetres.	Decimal parts of English inch.
1. Granular tubercles of the peritoneum,	0·005	·001968
2. Tubercular *corpuscula* from tubercular cervical glands, ...	0·005	·001968
3. *Corpuscula* from pulmonary tubercles of a male of ten years,......	0·0054	·00212498
4. *Corpuscula* of tubercles from the *pia mater* of a child,......................	0·005 —0·006	·001968 —·0023622

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* Recherches sur la Phthisie, 2d edition. Paris, 1843.

	Millimetres.	Decimal parts of English inch.
5. Tubercular *corpuscula* from the *pleura*, the *pericardium*, the bronchial glands, and the spleen of a child of three years and a half,..	0·005 —0·0075 long, by 0·005 broad	·001968 —·00295275
6. In pulmonary and intestinal tubercles of a child of nine years,......	0·005 —0·0075	·001968 —·00295275
7. From peritoneal tubercles of a man of forty-five years, ...	0·005 —0·01	·001968 —·003937
8. From the bronchial glands of a girl of thirteen years, ...	0·005 —0·01	·001968 —·003937
9. From a caseous tubercular infiltration between the arachnoid membrane and the *pia mater* of a child of four years,..................	0·0056—0·0084	·00220472—·00330708
10. From the bronchial glands of a child of six years, ...	0·006 —0·0072	·0023622 —·00283464
11. From pulmonary tubercles in a female of twenty-three years dead from pneumothorax,...........	0·006 —0·009	·0023622 —·0035433
12. From cerebral tubercles in a child of four years, ...	0·0067—0·0075	·00253779—·00295275
13. From cerebral tubercles in an adult, ...	0·0072—0·0096	·00283464—·00377952
14. From liver—tubercles in a female of sixty years,...	0·0075—0·01	·00295275—·003937
15. From tubercles of the cervical glands undergoing softening in a girl of sixteen years, ...	0·0075—0·01	·00295275—·003937
16. From a softened tubercle in the submucous tissue of the ileum in a child of ten years,........................	0·0075—0·01	·00295275—·003937

From one glance at these measurements, it results, that the size of these *corpuscula* may vary much, yet, for the most part, fluctuates between the extremes here assigned; and further, that different organs and ages exert an insignificant influence on the size of these *corpuscula*. We usually observe them, nevertheless, more easily in the yellow tubercle than in the gray granulations. At first, in general, difficulty is experienced in recognizing them and judging rightly of their details, because their structure is concealed by their close juxtaposition with each other, the closely-connecting intermediate matter, and the numerous surrounding granules. But when they are moistened in the yellow caseous tubercle with a little water, even in preparations originally dry, being first brought correctly into view with all their characters, with high magnifying powers, with good perpendicular diaphragm, with clear day-light or lamp-light, we then find them uniformly, in the most definite manner, in tubercular productions.

I here observe in passing, that exact and manifold measurements, corrected by different methods, appear to me indispensable in these delicate researches; because, notwithstanding the fluctuation between different extremes, the globules of the same substance may be always reduced to a fixed average size, of which the relative value to the size of the elements of other forms is very great.

Water causes no change in the tubercle-globules; vinegar renders them more transparent, without remarkably changing them, and furnishes, in this respect, a valuable means of diagnosis, because, by demonstrating the absence of *nuclei* in the tubercle-corpuscles, it forthwith allows us to distinguish nucleated-cells in many, namely, from those of purulent matter.

Ether and alkohol evince little action on the tubercular corpuscula. Concentrated ammonia renders them transparent, and partly dissolves the intergranular substance, on which the granules escape and may be then easily measured. In a concentrated solution of caustic potass, they are completely dissolved. In the concentrated mineral acids, viz., hydrochloric and sulphuric acid, they are equally dissolved, though more slowly.

This is the place to inquire what position ought to be assigned to tubercular-globules among cells of pathological character. While the perfectly-formed cell consists manifestly of a cell-membrane with cell-content, of one or more *nuclei* with nucleated *corpuscula*, manifold observations made on the pathological and physiological formation of cells have shown me that this formation is by no means a general one, and that to detach from the cells so many globules, not corresponding to this type, would lead in the end to useless fissures. The tubercular cells appear to me to belong to that simple form of cells in which a cell-membrane encloses a semifluid-content, and in this a certain amount of small granules. They resemble some masses of a sort of albuminous-globules which I have often found in different fluids, namely, in the erroneously-denominated lymph-effusions in inflammatory diseases of cachectic individuals. These globules are regularly spherical, very pale and transparent, and the granules contained in them are likewise transparent in the centre.

A somewhat more advanced stage in the process of formation is presented by these cells in effusions of inflammatory character from serous membranes, often presenting undeveloped non-nucleated pus-globules, which again form the transition to nucleated pus-globules.

The tubercular *corpuscula* are also cells remaining from a low or little advanced stage of development.

Before we proceed further, I must here solve another question in relation to diagnosis. Persons of the highest authority, among others Cruveilhier, have viewed the tubercular process as a modification of suppuration. (1.) Further, tubercular matter has been often described, especially by Cruveilhier, in cancerous swellings. That in these propositions error is often insinuated, I am firmly convinced. I have accordingly described in the Bulletin of the Anatomical Society of Paris,* a case in which, in a medullary tumour of the testicle, a substance altogether very similar to tubercle was formed, which, nevertheless, was by no means of the nature of tubercle. I have also observed in the Hospital St Antoine at Paris, another case, in which all the assembled physicians present believed that there was before them a case of cancer of the liver, while the microscope proved, evidently, nothing like *tuberculosis* of the liver. Lastly, I have observed several cases in which *tuberculosis* and cancer were mingled in the same organ. (2.) The question has hence often occurred to me, whether I could in all cases distinguish definite tubercular matter from pus and cancer? The following characters contain the affirmative answer in the most decided manner.

First of all, it is necessary to keep in view the difference between tubercle and pus.

Pus-globules are larger, and present in section a diameter varying from 0.01 ($\frac{01}{100}$ or $\frac{1}{1000}$) to 0.0125 ($\frac{0125}{10000}$) of a millimetre; they float free in more or less serum; their figure is round and globular; their surface slightly uneven, sometimes beset with fine granules; their cell-membrane is more or less transparent; in their interior is observed a moderately-homogeneous content, and sometimes one, sometimes two, sometimes three, rarely four or five *nuclei* with manifest outlines, of more or less roundish form, of from 0.0033 ($\frac{33}{10000}$) to 0.005 ($\frac{5}{1000}$) of a millimetre in diameter, occasionally containing a nucleated *corpusculum.* With high magnifying powers we distinguish these nuclei most beautifully without addition of vinegar; but this agent places their existence beyond doubt, because it proves in tubercular corpuscula, on the contrary, the absence of nuclei. The *pus-corpuscula* certainly vary at times, and may, in the case of malignant purulent matter, be changed in different modes by means of their serum. After some practice the observer may always distinguish them from tubercular substance, even in the occasionally difficult cases of tubercular softening, and which we are afterwards to notice again. We shall then specify also the diagnostic marks between concrete pus and softened tubercle.

The difference between tubercle-cells and cancer-cells is still more conspicuous. Not only the cancer-cells but also their *nuclei* are mostly larger than the globules of tubercle. The cells of skirrhus are from 0.0175 ($\frac{0175}{10000}$) to 0.2 ($\frac{2}{10}$), and sometimes 0.025 ($\frac{025}{1000}$) of a millimetre in diameter. Their outlines are irregular; their aspect pale and fine-granular; the *nuclei* of these cells have sharp, roundish outlines, and are from 0.0125 to 0.015 of a millimetre in diameter. These nuclei are often free, and a collection of a certain quantity of them may then present a great similarity to tubercles. I have also lately observed a case of the nature of skirrhus of the liver, in which a mass of free nucleated cells, mingled with many fat-granules, caused to me a momentary doubt; but the measurement of the *nuclei* and the persistence, with these of a certain quantity of perfect cells, quickly placed the diagnosis in a clear light.

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* Bulletins, &c., Dix-huitieme Annee, p. 80.

The globules of medullary fungus,* which, observed cursorily, are only cell-*nuclei*, are easily distinguished from tubercle-cells. They are from 0.01 to 0.015 of a millimetre in diameter; their edge is sharply defined externally, within it is less clear; and with fine-granular content, usually one to two, sometimes three, very evident nucleoli; their outlines are perfectly round or oval, oblong ellipsoidal; lastly, we very often observe, distinctly, the cell-membrane forming sometimes a globule from 0.015 to 0.02 in diameter, sometimes assuming the form of oblong cells, or caudate bodies. Some days ago I examined an *osteo-sarkoma* of the upper jaw, which was communicated to me by M. Velpeau, in which the medullary cells showed their purest type, of considerable circumference to about 0.032 of a millimetre in diameter. The cells of alveolar cancer present a diameter of from 0.0166 to 0.02 parts of a millimetre, and contain one to two *nuclei* of from 0.0063 to 0.0083 parts of a millimetre, in the interior of which is often seen a nucleated-corpuscle. They are moderately flat, and on the surface in general finely pointed. Besides, we find frequently in this species of cancer, large parent-cells which enclose several small ones, and still larger concentric cells to about 0.63 parts of a millimetre in diameter.

Tubercular matter contains therefore a perfectly peculiar cell-form, which may be distinguished with the naked eye very determinately by the practised observer from all similar structures.

We have next to describe the softening process in tubercle. But we must first analyze the mechanism of softening in a physiological point of view.

Without the aid of the microscope the softening process of tubercular matter cannot be rightly studied, since here, more than in any other form of this morbid structure, with the change in the tubercle itself, the observer has before him those of the surrounding structure, from which he ought carefully to distinguish them, in order to avoid falling into inextricable confusion.

In general, it may be said, that the changes in tubercular structure itself, abstracting from those in the surrounding parts, consist in this, that the solid connecting material itself becomes fluid; that the irregular tubercular globules lying close on each other become progressively more and more detached from each other, though clusters of connected masses are still met with; that further, these globules being partly free, become more rounded, more transparent and thinner; and that the dissolving *blastema* surrounding it becomes more granular.

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* By Medullary Fungus most German writers understand what is called in France and England *Enkephaloma*. At the same time, they do not in all cases use very definite language.

With the naked eye, as well as with the microscope, purulent matter may often be seen with the tubercular substance; but this matter—and the law appears to me of the greatest importance—never has its origin in the tubercle itself, but constantly in the parts immediately surrounding the tubercle. These several facts on softening may be rendered clearer by examples.

1. A female of thirty years, who presented in both lungs tubercles in different stages of development, had in the lower lobe of the right lung a mass of well circumscribed softened miliary tubercles. This, when carefully divided by a sharp knife, showed internally a yellow permanently open capsule of moderate consistence, and within the same some drops of whitish yellow moderately fluid matter, which with the naked eye resembled purulent matter; peculiar caseous tubercular substance was found only in scanty amount in the same. Under the microscope this fluid presented floating granules; and, partly still adhering to each other, but mostly isolated, floating globules of 0.0075 parts of a millimetre, partly transparent, containing in their substance imperfect opaque granules without inner nucleus anywhere.

Nowhere did I find pus globules in this fluid. Several very small capillary bronchial ramifications were filled with tubercular substance.

2. A child of six years, labouring under diarrhœa, and in whom the diffused miliary *tuberculosis*, as is not unfrequently the case, had given rise to typhoid symptoms, died in Bex in the month of October; and I inspected the body. In the lungs were found a quantity of miliary tubercles softened in the centre. I selected for examination those most isolated, and especially those which in their neighbourhood presented no inflamed pulmonary tissue. In the interior of this I found exactly the elements previously described,—a granular fluid, and floating in it the non-nucleated tubercular *corpuscula* carried to the surface, about 0.01 of a millimetre in diameter; no purulent matter; in many places remains of fibres of the pulmonary cells.

3. In the ileum of a child of three years, who presented in the bronchial glands crude and partly calcareous tubercles, and in the cerebellum a tubercle of moderate size, I found among others, in the cellular tissue, beneath the unaffected mucous membrane of the bowel, one tubercle perfectly enclosed on all sides, softened internally. The fluid contained in this tubercle consisted of a very granular thickish mass, in which floated inflated tubercular

corpuscula, though in small quantity. No trace of any purulent corpuscles was observed.

4. The softened pulmonary tubercles of a man of twenty-five years, in whom the pulmonic tissue around the tubercles was partly hepatized, and in whom some large softened tubercles communicated with the small bronchial tubes, presented the following elements:—1, Isolated tubercular corpuscula, more or less rounded; 2, globules partly broken down, dissolving in granules; 3, mutually adherent clusters of tubercular *corpuscula*, still more penetrated by granular intermediate matter than that is manifest in peculiar crude tubercle; 4, a quantity of granular fluid; 5, a mixture of floating pus-globules and tubercular corpuscles in almost equal proportions; and 6, a mixture of both those elements in all possible proportions.

5. In the beginning of July 1843, I extirpated from the neck of a young girl of sixteen years several enlarged cervical glands, as they began, by pressure on the jugular veins, to become dangerous. In these glands I found tubercular matter in all its different forms. But the softened tubercle was everywhere mixed with yellow, thickish, purulent matter. This case was well suited for illustrating the powers of the microscope for diagnosis. I placed both fluids in the different proportions of mixture under the compound microscope, and could easily distinguish the two elements, especially when I allowed a little acetic acid to be pressed by its capillarity between the two glass plates; there remained of the *pus-corpuscula* only the nuclei, while the tubercular *corpuscula* maintained their position in the field.

I have elsewhere several times observed in the cervical glands this mixture of purulent matter and tubercular substance.

6. A mixture of the elements of inflammation, suppuration, and tubercular softening, I found in a lung which was sent me by Professor Berard, surgeon to the Hospital of La Pitié, at Paris, in order to examine and ascertain the nature of the morbid elements, as the examination with the naked eye led to no result. This was the lung of a man of forty-five years, who, on account of a white swelling of the knee, had undergone amputation six weeks before death. The inferior lobe presented the characters of red hepatization; its texture was irregular, softened, compact, sinking in water. In many isolated spots of several millimetres in circumference, there were, in the midst of the hepatized substance, yellowish-white spots, which appeared on microscopical examination as suppurated lobules, and contained partly pus-globules and *nucleoli*, partly larger granular globules. There were found besides, in many spots of the inflamed pulmonic tissue, softened miliary tubercles, in which the compound microscope showed evident tubercular globules, though mingled with purulent elements. The vascular injection round these tubercles was

very considerable, and in many places the pulmonic texture appeared infiltrated at once with tubercular and purulent elements. The heart of this patient was adherent to the pericardium, and between the two was the remains of a former attack of pericarditis, organized pseudo-membrane, which in like manner presented miliary tubercles manifestly circumscribed.

This is one of those cases in which, without the most careful examination, and without accurate knowledge of minute anatomy, the distinction between the two elements would be impracticable; and had the preparation fallen into the hands of a candid, yet skeptical beginner, it would have led him only into confusion and doubt. This case shows further, in a convincing manner, that nature proceeds by no means in these productions in a mode so systematic as we might infer from our learned medical hand-books, and that he only begins to understand her who has followed her through her numerous proteiform changes.

But that softened tubercular substance may be in contact with destroyed textures, without being mingled with the elements of purulent matter, is proved by the history of the development of intestinal tubercles. Of this I adduce one among many examples. In a child of ten years, who died from tubercular perforation of the bowel, we found, besides pulmonary tubercles, a great number of intestinal ulcers, at the bottom of which lay the tubercular substance, partly still free and softened. Its constituent parts were the usual softened tubercular substance mingled with the elements of destroyed mucous membrane and muscular fibres; on the basis-surface of the ulcers were found here and there globules from 0.0125 to 0.015 of a millimetre in diameter, containing one or two *nuclei* of 0.005 of a millimetre in diameter,—most probably new epithelial cells. We never found, however, *pus-corpuscula*, which in general are much more rare in the intestinal mucous membrane than is usually allowed, and especially more rare than in other mucous membranes. From these intestinal diseases, however, an exception is observed in dysentery, in the discharges of which I have found with blood-corpuscles and mucus with epithelium, a large quantity of pus-corpuscles and granular globules.

8. I lastly adduce here an instance in which the like lungs presented the different forms and mixtures of softened tubercle. It is that of a female of twenty-four years, whom we saw in the wards of Andral, suffering under tubercular pneumothorax for nearly one month a course remarkably lengthened in this disease.

In the first lung were found, in the parts surrounding a very large cavity, softened tubercular masses of whitish-yellow colour, like thick purulent matter, and amidst these a small cavity surrounded by congested pulmonic tissue and inflamed bronchial tubes. In this fluid the observer saw most distinctly with the microscope the mixture of purulent matter with softened tubercle; the extremes of the two forms were very similar to each other; but nowhere was there seen any transition of the one into the other. Acetic acid proved here also, by its well known reaction, their diagnostic value.

But the resemblance which in such cases takes place between the two elements, leads us to the notice of an important pathological fact. This is, that when the last microscopical elements of two different pathological secreted products are similar to one another, this similarity always increases as it were in convergence, the less the eye is aided by instruments, and it attains its maximum with the naked eye of the unpractised observer, who sees most with the eye of fancy; and unhappily this is more a rule than an exception. The difference is so much more manifest and more considerable, in proportion to the more powerful and better defined microscopical magnifying power employed, and in proportion as the eye has been inured to the study of minute differences. From neglecting this circumstance, long discussions have often arisen in science; and, accordingly, no French physician of the present day yet believes in the difference between softened tubercle and purulent matter, though he has not employed the proper means of exact demonstration. The disregard of this fact is also one of the reasons of the long-continued discussion on mucus and purulent matter, to which we shall subsequently revert.

The microscope is also capable of distinguishing, in doubtful cases with external resemblance, whether the physician has to do with softened tubercle, with thick purulent matter, or with a mixture of the two.

The examination of the left lung was peculiarly instructive for the study of tubercular softening.

Besides cavities and crude tubercles, it contained tubercles taken in different conditions of softening. 1, Small masses of the size of a lentile, detached, and fluid, and softened in the centre. 2, Small cavities further advanced in softening, ranging from the diameter of a pea to that of a hazel-nut, partly communicating with bronchial ramifications. 3, In the pulmonary cellular tissue,* diffuse, infiltrated, yellow-grayish, softened masses of irregular shape. In all these different forms, the microscope demonstrated the existence of tubercular cells, swelled, enlarged to one-fourth of their diameter, containing clusters of granules in their substance. There were, besides, many free granules, and many tubercular *corpuscula* dissolving into granules taken in the process of solution. But purulent matter was found only in the softened parts which were in contact with inflamed or softened pulmonic texture, or with open bronchial branches. In the tubercles, on the other hand, mentioned under the third head, in which only the pulmonic cellular (filamentous) tissue was infiltrated, were found, besides the elements of tubercle, though more or less destroyed, pulmonary fibres, but never any trace of purulent matter.

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* This evidently means the pulmonic filamentous tissue.—Zellgewebe.

It appears, further, that the tubercular *corpuscula* are in purulent matter quickly disintegrated, and are dissolved into granular fluid, as we frequently find in the expectoration from unequivocally tubercular lungs and other ulcers no genuine tubercular *corpuscula*.

This point leads to one of the most frequent terminations of tubercular substance, its solution, and the wasting of its cells.

We see that tubercles pass through three different stages of development. In the first, we see the tubercular *corpuscula* deposited closely on each other, and compact in their interior. In the second, they begin to be separated, while their still increasing size is by no means an indication of progress; it is not growth, but the swelling of incipient disintegration; at the same time, the *blastema* we observe to become more fluid, thinner, and more abundant in molecular granules. In the third stage, which is also that of true disintegration, these globules, the internal cohesion of which is already very much impaired, are separated into a fine granular half fluid mass. In other respects, their course is similar to that of pus-globules in the healing process conducted by nature, in the absorption of manifest purulent effusions.

The third stage of *tuberculosis* is consequently not that of the formation of cavities, as the majority of pathologists admit, but that of the solution of its elements. The formation of cavities is only an operation of tubercle upon the surrounding tissue, by no means a change in the tubercle itself. There is, further, in all inquiries relating to the nature and tendency of tubercles, too great a disposition to attach importance to the descriptive characters of pulmonary tubercles. This leads to many errors, of which the very generally admitted idea of the insanability of *tuberculosis*, and, accordingly, of phthisis, erroneously yet constantly viewed as a consequence of tubercles, may be specified as one of considerable moment. The ulcer of the lungs, however, is physiologically not different from the tubercular ulcer of the intestine and skin, although this pathological process in the elastic substance of the lungs readily spreads much farther, and, by reason of the dignity of the organ, exercises a much more dangerous effect on the organism.

This is the place to mention a fourth condition or stage of tubercle which has not been correctly understood till of late.

This is that of calcareous transformation, probably more correctly the induration of tubercle, as the calcareous salts have been found in this state in smaller quantity, according to chemical analysis, than was previously believed. This was described by the older authors as *phthisis calculosa*, considered by the modern French physicians Papavoine, Rilliet and Barthez, E. Boudet, as one of the healing processes induced by nature; and, in this point of view, the observations of Rilliet and Barthez deserve attention, that in several acute diseases, especially abdominal typhus, *variola* and *scarlatina*, tubercles are healed by the process of calcareous transformation.

That the induration of tubercles, in truth, is one of the most important modes in which these bodies undergo the curative process, manifold observations have shown us. But of all proofs the most decided is that furnished by the microscopical structure. In the beginning of the calcareous transformation, we observe in them only a large quantity of disaggregated tubercular *corpuscula*; at this period are seen with the tubercular granules fine mineral molecular granules, which being whitish transparent internally, give a much more evident resistance to compression than the soft tubercular elements. The latter diminish, and in the same proportion the amorphous mineral productions increase; the tubercle becomes progressively more solid, more granular, and more dry; it partly shrinks, and hence becomes an innocuous formation. Besides the amorphous mineral masses, I have several times found manifest cholesterine crystals in appreciable quantity in calcarized tubercles, and they also often contain infiltrated melanotic pigment matter.

A second argument, not less important for the truth of this proposition, is found in the mode in which this form of tubercle comes on. This we certainly often find in the bodies of persons destroyed by protracted *tuberculosis*; and here the sanative powers of nature encounter too great resistance, and operate only partially. Yet, not unfrequently I have found in bodies which have been cut off by sundry acute diseases, traces of long-continued and yet cured *tuberculosis*. The tubercles, indeed, not numerous in these cases, I always found in the condition of hardening. I recall particularly three instances, in which, after the most careful examination, I found not only few, but only one individual tubercle in the lung. The first was observed in a young female, who having died from abdominal typhus, presented in the upper lobe of the left lung, one single tubercle the size of a pea, which had undergone the calcareous transformation. The second occurred in a child of six years, who died of gray hepatization of the lung, the upper lobe of the right lung of whom contained one single tubercle. Lastly, the third case was that of a man of fifty years, who sank under pleuritic effusion with encysted empyema, and in whom the right lung presented at its surface one single calcareous tubercle six millimetres in diameter, ($\frac{23}{100}$ of one English inch, a little more than two lines), the interior of which contained granular powder, organic ligamentous matter, colouring matter, and cholesterine crystals.

That calcareous tubercles are formed in different organs, recent observations concur in proving. I have found them not only in pulmonary tubercles, but in tubercles of the bronchial and mesenteric glands.

The analysis of calcarized tubercles, given by Boudet, deserves attention. He found, besides a small quantity of carbonate and phosphate of lime, some hydrochlorate of soda and sulphate of soda. Thus,

Hydrochlorate of soda, - -	0.400
Sulphate of soda, - - -	0.288
	0.688 in 1000 parts.

To these chemical constituents may be added cholesterine, which, as already mentioned, I have not uncommonly met in calcarized tubercle.

Besides the essential constituents of tubercle, we find further several elements, which, without being uniformly present, are nevertheless frequently met with in these bodies.

First to be noticed comes the pigment infiltration, namely, *Melanosis.* It is found, as in other pathological textures, principally in three different forms; 1, as a granular infiltration; 2, contained in individual globules of the diameter of 0.016 of a millemetre to 0.024 of a millimetre, in rare cases amounting to 0.033; and, 3, in various other elementary cells. Not unfrequently, also, we find it in the interior of the epithelial cells mixed with the expectoration. Its appearance within and around the tubercles is indicated frequently, particularly in the lungs, in which, besides the internal general causes, the state and occupation of patients further favours its formation. Hence, for instance, it is frequently found in coal-burners, and in them Lecanu has clearly proved the presence of carbonaceous matter. In the lungs, it is further found along with gray semitransparent miliary tubercles in calcareous tubercles, and the walls of purulent cavities, more especially in the bronchial glands, and not unfrequently in the expectoration of phthisical patients.

At the inner surface of the *ileum* and of the *cæcum,* melanotic matter is often strongly developed; and here it occurs as a flat or laminated infiltration, sometimes in growths projecting within the bowel; and in one instance we found, at the inner surface of the ileum, a pedunculated tumour of the size of a small hazel-nut, composed entirely of *melanoma,* tubercle, and blood-vessels.

Lastly, in tubercles of the peritoneum we have almost uniformly observed it; and in the body of a man of forty-five years, belonging to Bex, we found it in quantity so manifest, that the *peritoneum*, which had undergone the tubercular degeneration in a great degree, had acquired throughout a black and yellowish spotted aspect.

Fat is not unfrequently found in tubercles, usually in the form of the well-known opalescent adipose vesicles.

Fibres are often found mingled with tubercles. These belong, however, most rarely, to the pathological secretion, and are mostly no more than the elementary fibres of the part, between which the tubercle has been deposited. Thus we find in the tubercle of the serous membranes fibres of the *pia mater*, of the *pleura*, and the *peritoneum*, and in the gray miliary tubercles of the lung, the well-known elastic fibres of the pulmonic tissue. But the distinction of tubercle into fibrinous tubercle and albuminous tubercle which Professor Gerber of Bern has made, appears to me to be artificial; and genuine fibrous structure in tubercle itself as a fibrous secretion appears to me to belong to exceptional phenomena. Once, indeed, I found in the body of a child of three years, not only in the tubercles of the cerebral membranes, of the lungs, and of the *peritoneum*, but also in those of the liver and the kidneys, true fibrous structure; with these granular irregular fibres were found also caudate cells; the tubercular *corpuscula* presented nothing peculiar. But this case belongs to the rare exceptions.

Crystals are occasionally found, but only exceptionally in tubercles. Once I found three-sided prismatic crystals in pulmonary tubercles; in another case the same in tubercles of the bronchial glands; and in a third case cholesterine-tables in softened cervical glands. I do not mention here naturally the crystals already noticed found in the calcareous tubercles.

Olive-coloured moderately large globules, from 0.016 to 0.025 parts of one millimetre in diameter, containing from three to four small globules, I found once in a mass of bronchial and pulmonary tubercles, which I examined with the microscope. These were from the body of a female of twelve years, who died of pulmonary tubercles in the hospital for children at Paris.

Besides the elements now specified as actually found in tubercle, the observer often sees under the microscope, cells which, proceeding from the enclosing pulmonary texture, are only accidentally mixed with them, and by no means belong to the peculiar tubercular substance. I have already mentioned in detail the *pus-corpuscula* in this point of view. One element frequently mixed with the same are the granular globules from 0.02 to 0.025 parts of a millimetre in diameter, more or less filled with yellowish granules, containing sometimes a *nucleus.* These are found particularly in inflamed and softened pulmonic tissue.

An element frequently appearing, and readily causing illusion, are new epithelial cells, produced by the division of the small capillary tubes, from 0.0125 to 0.015 parts of a millimetre in size, of irregular roundish shape, enclosing a *nucleus* of 0.005 parts of a millimetre in diameter, which sometimes contains in its interior a *nucleolus*, but has usually a fine granular content. These cells are found in preparations around tubercular masses, but never in the tubercles themselves,—a certain proof of their foreign origin. Not unfrequently we find also among these new epithelial cells, cylinder-epithelium, even ciliary cells here and there. Caudate *corpuscula,* with or without *nuclei*, are also not rarely mixed with tubercles, especially those which are found in the vicinity of the serous membranes.

The true constantly-existing constituent parts of tubercle are, therefore, granules, ligamentous or connecting tissue, and the characteristic tubercle-cells. Tubercle appears first after its separation from the blood in a compact form, afterwards in the softened state; this proceeds either to disintegration, or, in the course of healing, to the calcareous state. As irregular or not constant elements in tubercle are found *melanosis*, the most common adjunct, then fat, fibre, and olive-coloured dark globules and crystals. Lastly, we frequently find, accidentally mixed with tubercle, the products of inflammation, namely, exudation, and suppuration, and the elements of epithelial scales in multiplied forms.

SECTION II.—UPON THE PRINCIPAL FORMS AND MODES OF DEVELOPMENT OF TUBERCULOSIS IN DIFFERENT ORGANS.

Though in general it is an extreme view always to think of pulmonary consumption when the subject of tubercles is under consideration, it is nevertheless certain that the lungs are, of all the organs, those in which tubercular formations occur most frequently, and are most fatal. We shall afterwards advert to the law announced by Louis, according to which, when in any one organ tubercles take place, at least in adults, they also are contained in the lungs.

I in other respects understand, as previously known in general in this inquiry, the facts communicated by our most able writers on pathological anatomy, Laennec, Louis, Carswell, Rokitansky, and others, on *tuberculosis;* and I confine my attention particularly to those points upon which minute pathological anatomy, and microscopical observation, furnish conclusions. I therefore begin this section with some of the most important facts illustrating *tuberculosis* of the lungs, and I shall then bring under view the morbid process in various other organs according to those conditional peculiarities which depend on different parts affected.

I. SOME OBSERVATIONS ON PULMONARY TUBERCLES.

The points which I am here to touch are;—1, the state of the pulmonic tissue in the part surrounding tubercles; 2, the nature of gray granulation; 3, the minute elements of pulmonary ulcers; 4, the nature of the expectoration in pulmonary tubercles; 5, the integrity of the lungs in tuberculosis more or less general; and 6, the changes of the pleura in *tuberculosis*, and the supplemental circulation established in that membrane.

1. *On the State of the Pulmonic Tissue around Tubercles.*

There are cases, especially in aged persons, in which the pulmonic tissue around tubercles undergoes at first almost no change; and even at a subsequent period, though, from the development and diffusion of the tubercles, the phenomena of diminution and partial absorption of the compressed tissue ensue, yet they produce no inflammatory symptoms, and accordingly little reaction on the general organism. These are cases in which pulmonary consumption observes a most chronic course, and may last during many years. In a very great number of cases, on the other hand, the pulmonic tissue undergoes not only a mechanical alteration from pressure, but is, by the inflammation there developed, rendered useless for the exercise of its necessary function of maintaining life, and in this mode chiefly induces death. Of this inflammation the cause is twofold. On the one hand, the tubercles, like foreign bodies, irritate the surrounding tissue; on the other hand, they compress, being normally void of vessels, wherever they are formed, the existing capillary vessels; and those which are left, being greatly reduced in quantity, and over-distended with blood, are easily disposed to inflammation. Though I must here mention briefly the seat of tubercles in the lungs, I have however to return, in the third section of this essay, expressly to this subject. I have here only to observe by the way, that, by most careful dissection and microscopical examination, I have ascertained that the original place of the separation of tubercle from the blood is mostly in the elastic pulmonary tissue, and between its fibres; that, nevertheless, the secretion of tubercular matter also takes place, though more rarely, also within the capillary bronchial tubes and the pulmonic vesicles; that, accordingly, neither the pulmonic cellular (filamentous) tissue nor the *vesiculæ* exclusively are the seat of tubercles, but that these may be effused in both situations. Further, as soon as the tubercles in the lungs are only one millimetre in size, they affect both tissues, as the pulmonic cells in their natural state lie too near each other, not to be partly destroyed by the extension of individual tubercles.

The pulmonic tissue surrounding tubercles is therefore rarely sound. When it is diseased, as usually happens, the nature of this morbid state is decidedly of inflammatory character, and, indeed, the inflammation is either lobular, or one more general extended over a certain number of pulmonary lobules. We find in these two forms, most frequently the characters of red hapatization, sometimes of gray hepatization. The observer should nevertheless be cautious in forming his opinion, and not always ascribe to the infiltrated pulmonic tissue the purulent matter proceeding from tuberculated lungs. Very often it issues from the small bronchial ramifications. The ordinary hepatized pulmonic tissue sinks to the bottom in water; is red, granular, softened, easily lacerable; the red colour borders on brownish; the pulmonic vesicles are at first difficult to be distinguished, yet become more distinct by pressure and washing. As we have already seen, a manifest deposit of colouring matter takes place round the tubercles. Occasionally the pulmonic tissue is so compact, that the smallest vessels, divided in the examination, remain open on the surface of the section. The inflamed pulmonic tissue is in other respects by no means separated from the tubercles by any proper capsule enclosing it. In many places we see the pulmonic vesicles entirely filled with the exuded products. In them, exactly as in diseased red pulmonic tissue, we find a very great quantity of granular globules, from 0.016 to 0.025 parts of a millimetre in diameter, quite filled with small globules of from 0.002 to 0.0025 parts of a millimetre in diameter. Sometimes are found in these different exudation-products, adipose vesicles, and small cells like pus-globules, of 0.001 millimetre in diameter, containing no nucleus, but within their substance, in small quantity, transparent granules of 0.002 parts of a millimetre in diameter. It is very important to observe, that the proper tubercular granules never appear in the midst of these globules beyond the tubercles; and, accordingly, in sections, the products of inflammation and of *tuberculosis* are met separate throughout. The single exception to this arrangement which has hitherto occurred to me, has been already mentioned above. The red softening of the lung may often be very extensively diffused. In one instance, in the body of a man of thirty-three years, in whom phthisis had constantly presented in its whole course, inflammatory symptoms, I found almost the whole right lung, containing many tubercles, hepatized. The left lung was less diseased, and showed only lobular pneumonia in several places.

In rare cases we find around the tubercles another very remarkable form of hepatization. This extends usually over a large portion of the pulmonary tissue, which is much more solid than in the ordinary hepatization, and presents a moderately uniform yellow-reddish aspect. In the tissue, we find only a small quantity of large granular globules; but, as the leading element of pathological infiltration, small globules from 0.01 to 0.015 parts of a millimetre in diameter, presenting in their substance fine granules, spherical throughout, not adhering to each other, not changing under acetic acid, showing no *nuclei* in their interior; in the larger aggregated globules appearing along with them, we find not unfrequently one, sometimes two *nuclei;* while delicate sections of this tissue are often yellowish-white, flocculent, and areolar. In many spots we find more fibrinous formations, with a striated aspect, and containing caudate bodies. In many spots also, the pulmonary vesicles are seen evidently filled with exudation. Amidst the chronically inflamed less vascular tissue, we often find small vascular spots of acute inflammation quite circumscribed. As to the different degrees of consistence in the inflamed pulmonic tissue, my own researches have led me to conclude that the inflammatory exudation contained in the pulmonic tissue, when it contains much fibrinous matter and few globules, gives an increased degree of consistence; but that the part is of diminished consistence, when it is either surrounded, on the one hand, with many globules, or, on the other, with a quantity of fluid *blastema.* Numerous globules, with a partly absorbed blastema, furnish a medium degree of hardness.

The pulmonic tissue around the tubercles may be compact without inflammation; and this is especially the case in more or less considerable effusion within the pleura. The lung is then much compressed backwards at the upper region of the chest. The microscope confirms the diagnosis, because it proves, in the simply compressed pulmonic tissue, the absence of the different exudation products mentioned.

The pulmonic tissue surrounding tubercles may be either sound with almost natural expansion, or sound but compact from compression, or inflamed; in the last case, hepatization, most frequently with red softening of the lung, is observed; but sometimes also a more yellowish form of hepatization, with increased consistence of the lungs, is recognized.

I shall, in the physiological exposition of *tuberculosis,* revert to the relation between that and inflammation. I here only observe, by the way, that I consider both morbid processes as throughout different, and that though tubercles often precede inflammation, they are never a product of inflammation.

2. *On Gray Granulations in the Lungs.*

Since the beginning of our investigations on the nature of tubercles, the gray semitransparent granulations, which are so frequently met with in the lungs of phthisical persons, have attracted attention so much the more, as the views on their pathological indication were various. One party considered them to constitute a peculiar form of consumption; and this doctrine, originally maintained by Bayle, at present numbers many adherents. Other pathologists, and we may here quote the imposing authorities of Laennec and Louis, regard them as forming the first stage of tubercles, which these observers consider as a transformation of the gray substance. We regard the view of transformation in general as incorrect, and belonging to the infancy of pathological anatomy. Besides this, microscopical investigation has taught us, that the gray semitransparent granulations already contain, most evidently, the characteristic elements of tubercles; and are therefore actual tubercles.

Usually transparent at the edge, they, for the most part, show clearly in the centre a yellowish turbidity. They are surrounded by no peculiar capsule, but by a manifest zone of vessels; indeed, I once traced a vessel into their interior. This vascular injection depends not on inflammation, but has its origin in this circumstance, that the granulations which are usually very much disseminated, compress a mass of small vessels, and hence cause a greater amount of blood to be found in their vicinity. In the interior of these granulations we have found pulmonary fibres, and always in considerable quantity, and among these most distinctly tubercular *corpuscula* in considerable quantity, but surrounded by a *blastema* rather transparent than granular. On the one hand, the transparency is hereby favoured; on the other hand, it is impaired by the still-existing and afterwards destroyed pulmonary fibres, compressed by the closely-compacted tubercular globules, as this takes place in the centre sooner than in the circumference; the first yellowish turbidity is found also in that spot, and it thence spreads over the whole granulation. Quite the same relation and mutual communication of tubercular-cells through the quantity of uninjured fibres, mixing of a hyaline *blastema*, are found particularly manifest in tubercles of the serous membranes, as the *pia mater*, pleura, and peritoneum.

But that the gray semitransparent granulations are not a necessary formation-stage of *tuberculosis*, we see from this fact, that we often find very small, almost microscopical tubercles, of yellow caseous aspect. This is particularly the case in that form of miliary tubercles, which, as appearing invested by a yellow hardish membrane, seem probably to be originally deposited in the pulmonic vesicles.

In other respects, the attentive inquirer, and especially whoever has had occasion to make many inspections of bodies, almost never finds the gray semitransparent granulations alone. We usually see at the same time, yellowish granulations, small yellow or large caseous tubercles, softened tubercles, and pulmonary ulcers. Cases nevertheless occur in which the whole lungs and other organs contain a countless assemblage of gray tubercles, which speedily cause death, before these ulcers have been completely formed. It is not at all rare to see large portions of the pulmonic tissue, closely compressed to each other, almost filled with gray granulations.

Occasionally in one organ or in one tissue, gray granulations are formed, and in an organ quite close, caseous tubercles are deposited. Thus, in a child of two years, I found beneath the arachnoid membrane, yellow-coloured tubercular infiltration, and, on the contrary, gray granulations beneath the *pia mater*. In another case, gray granulations were found beneath the *pleura*, and yellow, caseous tubercles in other organs, viz., the lungs, the bronchial glands, the pericardium, and in the spleen. In a third case, gray granulations were found under the *pleura* and *peritoneum*, and yellow tubercles in the mesentery, in the lungs, in the liver, and in the kidneys. In general the granulations are found deposited most frequently in the cellular tissue beneath the serous membranes, and in the pulmonary tissue between the elastic cell-fibres, usually in that situation accompanied with much black colouring matter; a circumstance which, on the one hand, often renders the gray aspect more characteristic, but, on the other, not unfrequently conceals the recognition of them from the unpractised eye, as I have often satisfied myself both in hospital and private practice.

The pulmonic tissue surrounding the granulations is often affected with inflammation, and then microscopical inspection shows the well known aggregated globules, in the circumference of the tubercles. The doctrine that *tuberculosis* arises from inflammation has been often promulgated. Recently, Rilliet and Barthez have availed themselves of this doctrine, as to the gray semitransparent granulations, as stated in the following passage:—"Thus gray granulations and infiltration take place in consequence of inflammation, but only in tubercular subjects, and both may give rise to yellow tubercular matter."* In a similar manner the authors express themselves in another passage.†

In this view I can by no means join; because, on the one hand, exact microscopical examination never shows any transition, any intermediate formation between the products of inflammation and tubercles; and, on the other side, the authors themselves allow that they have found these lesions only in bodies affected with tubercles. Pulmonary inflammation again, in its different forms, and without the presence of tubercles, is so frequent a disease, that this fact alone contradicts the truth of the doctrine. That tubercles may be deposited in inflamed lungs is not extraordinary;—but the fact by no means prove that they owe their origin to inflammation, and are only a transformed product of that process.

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* Traité Clinique et Pratique des Maladies des Enfans par Rilliet et Barthez. Paris, 1843. Vol. iii. p. 28.

† Ibid. Vol. iii. p. 223.

As a general summary of all that has been said, I may here state my opinion on the nature of the gray semitransparent granulations;—that they are in all respects one form of tubercle; that they often serve as the origin of the yellow caseous tubercle, but that the latter do not necessarily arise from these gray ganulations, but may often have from the first their own peculiar aspect; that, lastly, in generally diffused *tuberculosis*, the gray granulations, existing in considerable number, may cause death before they have passed through their further stages of development.

3. *On Tubercular Ulcers of the Lungs.*

The old physicians employed the correct expression, *ulcer of the lungs*, to designate the ulcerous destruction of the pulmonic tissue effected by tubercles.

This expression appears to me more suitable than that of caverns, excavations, purulent cavities, and similar terms. Had we here to treat of mere terminology, this remark would be superfluous. But we have to consider one of the most important subjects in pathology. As we undertook to controvert the doctrine, that tubercular softening is a process of suppuration, so we regard the origin of pulmonary ulcers, and the process of ulceration in general, as altogether different from that of suppuration. It is assuredly established that suppuration very often terminates in ulceration; but we observe in several of the mucous membranes, especially in those of the *bronchi*, as in *bronchitis* and *bronchopneumonia*, a considerable quantity of pus-globules separated without any trace of ulceration taking place. A similar fact we observe in catarrh of the uterus, and in general in many superficial mucous inflammations. On the other hand, we see ulceration without suppuration, as is particularly observed in intestinal ulcers, in which normally we find no purulent matter. Hunter has, if I mistake not, represented the ulcerative process as a peculiar termination of inflammation. This form of ulceration appears to me to depend on the inaccessibility of the small capillary vessels to the circulation. By the matter in abscesses, the circulation in a certain portion of the small capillary vessels is interrupted, and consequently the parts become destroyed rather mechanically than chemically; they fester, as they are no longer nourished; become foreign to the organism, separated and detached from it. The transition from ulceration to gangrene is quite imperceptible. This furnishes the explanation of the dead masses of cellular tissue in boil, the ulcers of artificial origin from gangrenous destruction of part of the skin by caustics, and many other pathological and therapeutic conditions. The pulmonary ulcer in tubercles, which, physiologically, is little different from the intestinal ulcer, arises first from softening of the tubercular substance and destruction of the pulmonic tissue, partly from pressure and absorption, partly from obliteration of a set of small vessels. That suppuration may here co-operate, we can by no means deny; but we deny that it is the sole cause of the formation of ulcers; to what extent it is in the lungs a constant consequence of ulceration we shall afterwards see.

That, however, in general, *tuberculosis*, a true ulcerous diathesis, though not one depending immediately on tubercles, takes place, appears from the valuable results of the observations of Louis, according to which, in a third part of the bodies of phthisical patients, ulcerations are found in the air-passages, and in one-fifth part in the larynx and in the epiglottis;* and Louis expressly observes that these ulcers are not produced by tubercles. "I have" he says, "in no case met with tubercular granulations in the substance or at the surface of the epiglottis, of the larynx, or of the windpipe, so that we must regard inflammation as the most frequent exciting cause of the ulcerations observed in these parts." That, further, these ulcers may arise under the general influence of the tubercular process, results from this fact, that Louis, among 180 inspections of bodies dead by other diseases in the larynx and windpipe, found only one case of ulceration in the larynx, and two instances of ulcerations in the larynx and windpipe. Since the year 1825, when the first edition of his work appeared, Louis established the truth of this proposition by this fact, that among 500 bodies of persons dead by chronic diseases not tubercular, he found no instance of these ulcers. From this list he of course excludes syphilitic disorders.

In order to estimate rightly the peculiar nature and indication of the different phenomena met with in pulmonary ulcers, the exact knowledge of all the elements taking place in them is particularly necessary; and these elements are much more multiplied than observation with the naked eye allows to conjecture. We have here to consider, first the innermost fluid, then the coagulated layer surrounding it, and then we have to turn our attention to the proper wall of the cavern and the surrounding pulmonic tissue.

1. The fluid which covers the inner layer of pulmonary ulcers,

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* Louis, Recherches Anatomiques Pathologiques et Therapeutiques sur la Phthisie. Deuxieme Edition. Paris, 1843, pp. 47—51 et 178.

usually of yellowish, gray, or yellow-red aspect, moderately thick, consists of the following elements · *a.* Peculiar tubercular matter is found in different forms; sometimes the tubercular globules in a state of integrity, most commonly attenuated by softening, being always in greater or less amount in the state of liquefaction, and consequently presenting a smaller granular mass. The pulmonary ulcer appears to be the proper decomposed assemblage of these tubercular cells. *b.* Pus-globules are always found in excavations, sometimes in very small quantity, though forming in sections a chief element of the fluid of cavities. The greatest part of the purulent expectoration proceeds from the *bronchi*, and not from cavities. *c.* Sometimes we find the small non-nucleated puriform albuminous globules, which probably are nothing else than imperfectly formed pus-cells. *d.* The large aggregate-globules, from 0.02 to 0.03 parts of a millimetre in diameter, are not unfrequently separated through the inflammatory softening of the pulmonic tissue, and mingled with the fluid of ulcers. *e.* We find purulent mucus, transparent, very viscid and ropy, in considerable quantity. *f.* Blood-globules are, in the frequent hemorrhages taking place in excavations from erosion of capillaries, often mixed with the different elements. *g.* Frequently we find tolerably well preserved pulmonary fibres, presenting often in sufficient compass uninjured *areolæ,* filled with tubercular *corpuscula. h.* Black colouring matter is found in this fluid, and communicates to it a gray colour. *i.* Epithelium is usually found in it in moderate quantity, partly plaster-epithelium, epithelial scales, and isolated cell-nuclei, partly cylinder-epithelium and ciliated cells. (3.) *k.* Twice I have found crystals, namely, three-sided prisms, in the interior of cavities. *l.* Lastly, in one case, I found a quantity of adipose tissue, and among others an entire piece composed of fat cells, from 0.015 to .002 parts of a millimetre in size, lying by each other.

2. Among this thick fluid, composed of very different parts, are found, not always, yet frequently, coarse pseudo-membranes, forming connected coverings, of elastic consistence, yellowish, presenting a laminated fibrous structure, and usually enclosing pus-globules in greater or less quantity. They appear to consist chiefly of coagulated fibrinous matter, with purulent elements. Sometimes they are less consistent, and present then a jelly-like aspect. They rarely line the whole internal wall of a cavity. They are attached usually, not firmly, to the subjacent tissue, and they show no new formation of vessels.

3. The most important element of the interior of pulmonary ulcers is the organized purulent membrane (*membrana pyogena*), usually found under the pseudo-membranes. This is intimately connected by minute blood-vessels with the subjacent pulmonic tissue, and presents a reddish, velvet-like, vascular appearance upon

the surface directed to the pseudo-membrane. By microscopical inspection we find in it an assemblage of capillary vessels, which appear, however, never to possess a proper separate circulation, but are connected with the subjacent pulmonary vessels. The substance of the membrane, the substratum of these vessels, presents an indistinct fibrous structure, with a quantity of small globules between the fibres. In the predominance of fibrous tissue and the small degree of vascularity, this may sometimes assume a cartilaginous appearance, and may therefore be often compared to cartilage, of which, however, no trace is found in this membrane.

Occasionally this newly formed organized membrane is observed in tolerably considerable extension, and lines almost the whole inner surface to the *bronchi*, to the mucous membrane of which it has a certain resemblance. But usually, by the incessant new separation of tubercles from the blood, its perfect organization is destroyed, and its tissue is lacerated, as we have often had occasion to observe. I found it completely organized in pulmonary ulcers, which were taken in the process of healing; and in general we regard the formation of this organized membrane as an healing effort of nature, in order, on the one hand, to protect by covering the pulmonic tissue exposed by ulceration to the operation of the air; and, on the other, to heal, by cicatrization, the cavity thus detached.

4. In the pulmonic tissue, which is situate immediately beneath this organized membrane, often exists a considerable amount of newly-formed cell-fibres, and of caudate bodies passing into these fibres. This tissue is found particularly beneath the thickened *pleura*, between which and the surface of the cavity there is often a very slender tissue-pellicle.

5. Lastly, the pulmonic tissue, which surrounds the cavities, is usually in a more or less irritated condition, and contains, for the most part, a number of crude recently-secreted tubercles, partly in the form of gray granulations, partly in that of yellow tubercular matter. This after-crop of tubercular eruption it is especially, which prevents the healing process of pulmonary ulcers, and incessantly anew interrupts and lacerates the isolated pellicle as it is forming, and always destroys more and more the tissue in which they are deposited. The pulmonic vesicles soon disappear in this morbid pulmonary parenchyma; the fibres stand out a little longer, but are also afterwards destroyed, as their appearance in the fluid of cavities and in the expectoration proves.

The cicatrization of hollow ulcers of the lungs may be effected in three different modes. The first is the separation of the cavity from the rest of the pulmonic tissue by the complete organization of the organized membrane of its inner surface. The latter communicates with one or more bronchial ramifications, and the fluid secreted within it, consisting of mucus and purulent matter, is easily expectorated. Occasionally the pulmonary ulcer remains stationary at this stage of imperfect healing; and in that case the secreting cavity furnishes in some sort an internal issue or drain. But this healing process may make further progress, and the vessels of the membrane become constantly smaller, their tissue become more fibrous, their range progressively more contracted, and thus at length form a cicatrix situate at the termination of a bronchial ramification. Another mode of healing consists in the effusion of a fibrinous mass, which progressively becomes organized; and thus at length a mass of fibrous tissue indicates the spot of the previous cavity. The observer must, however, take care not to regard as a cicatrix the remains of an effusion of blood into the pulmonic tissue, in which the decolorized compact fibrinous matter certainly presents a great similitude to the fibrous cicatrized cavity. The opening of a bronchial tube in the fibrous mass is, in these cases, regarded as a certain diagnostic mark. This is, however, deceptive, as the bronchial branches, no longer performing their function, contract and almost disappear. Lastly, the third mode of cicatrization, of which Rilliet and Barthez quote an instance,* consists in deposition of calcareous matter in the cavity, and deposition of fibrous cicatrizing tissue all round it.

To render this important subject,—the elements of cavernous pulmonary ulcers and their healing,—more intelligible, I adduce here some examples.

1. A pretty large cavity, of about two inches in diameter, in the apex of the right lung of a child of twelve years, contained in its surrounding parts a thick brownish-red pulmonary tissue, infiltrated with crude tubercles. The inner surface of the cavity was most irregular; and in several places were processes of pulmonary substance, which, almost isolated, were little attached to the lung. Part of the inner wall of the cavity was lined by an organized membrane, which consisted of vessels in a fibrous *stroma*, and which at many points presented a lacerated surface, in which the loss of substance was supplied by small crude miliary tubercles. Between these fibres of the membrane were seen globules of 0.0067 parts of a millimetre in diameter. The surface of the membrane was covered with a layer of yellow-red thick fluid, several millimetres thick, in which were found blood-globules, decomposed tubercles, *pus*-globules, pulmonic fibres, aggregate-globules, and crystals.

2. In the apex of the right lung of a child of eight years, I found a hollow ulcer as large as a fowl's egg, surrounded by pulmonic tissue, hardened, infiltrated with tuberculated matter, and containing much black colouring matter. In the fluid were found

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* Traité Clinique et Pratique des Maladies des Enfans, p. 238. Paris, 1843.

many fat-vesicles, and among others a portion of adipose tissue, consisting of connected adipose cells. All the tubercles in the different organs of this child presented much fatty matter, which was quickly dissolved in cold, and especially warm ether. The greater part of this fluid consisted of a viscid ropy mucus, in which only few *pus*-globules, but a good deal of epithelium, especially cylinder-epithelium, were found, besides many pulmonic fibres, granules, aggregate-globules, and a few tubercular *corpuscula*. The organized membrane was found in different spots.

3. In the left lung of a child of nine years, I found a pretty extensive cavity, which was partly filled with a green-yellowish fluid, consisting of many granules, large and small aggregation-globules, and pus-cells in smaller quantity. Amidst the fluid mass were found false membranes of elastic consistence, composed of coagulated fibrine, and *pus-corpuscula* enclosed in it. The walls of the cavity, anteriorly and above, were scarcely four millimetres thick, two-thirds of which consisted of false membranes, and caseous, softened, tubercular matter. In the other remaining layer, which was scarcely one millimetre and a half thick, were found no pulmonic vesicles, only a few pulmonic fibres, and particularly much fibrous tissue, consisting partly of caudate *corpuscula;* some of which enclosed *nuclei*, 0.005 parts of a millimetre in diameter. Right through the middle of the cavity passed a considerable vessel, to which a little destroyed pulmonic tissue was attached, and within which the blood was still partly fluid.

4. In a cavity of considerable size, occupying almost the fourth part of the lung in a female of thirty years, I found many membrane-like fibrinous clots, enclosing purulent matter, and covered with dissolving tubercular globules. The irregularly-organised pellicle presented in many spots so thick fibrousness that it has almost a cartilaginous appearance; in other spots it was much reddened and velvet-like. In this the observer recognized, by means of the microscope, an assemblage of small vessels, and besides these a gray arborescent network, which I took at first for lymph-vessels, the existence of which in tubercles Schroeder van der Kolk has demonstrated by injection. It turned out, however, that this was only a network of pulmonary fibres, destroyed and altered by softening. The smallest blood-vessels of the pyogenous membrane were from $\frac{1}{30}$ to $\frac{1}{40}$ of one millimetre in diameter; the blood-globules in these were unchanged. Proper tubercular elements I could not recognize. In the compressed fibrous tissue of the membrane were found caudate *corpuscula*. The surface of the cavity was most irregular and uneven; in some spots were found, in the vicinity of a bronchial tube, ciliary epithelial remains, several of which evinced ciliary motions forty-eight hours after death. *Pus-corpuscula* and blood-glo-

bules were found in considerable quantity in several spots of the cavity.

5. The considerable extent of this cavity recals to my remembrance an excavation in the upper part of the lung of an ape which I dissected in the year 1835, and which occupied almost two-fifths of the lung. Many of the apes of the *Jardin des Plantes* at Paris, it is well known, die from *tuberculosis*. I had then no opportunity of examining microscopically the yellow, green, thick fluid which the cavity contained.

6. In the body of a female of thirty-two years, I found a cavity the size of a walnut, which, communicating with several *bronchi*, was almost wholly lined by a velvet-like, vascular, fibrous membrane. Small globules were found between the fibres. This membrane had almost the appearance of a mucous membrane. On it were found only very few miliary tubercles. This cavity appeared to be taken in the beginning of healing.

7. In another lung, in which only few calcareous tubercles were contained, I found, at the end of a bronchial tube as thick as a quill, a cavity of about nine lines in diameter, which was entirely lined by a fibrous membrane, containing much black pigment, and with few vessels. This is manifestly a pulmonary ulcer near healing.

8. In the passage already quoted from the work of Rilliet and Barthez is the following description of a healed cavity:—"The base of the upper lobe is separated from the apex by a transverse fissure well depressed. A section through this point showed at the bottom of the fissure, and in the internal substance of the lung, a tubercular nut as large as a pea, completely encysted and cretaceous. In this nut terminated a zone of fibrous tissue half a centimetre broad, and one centimetre deep. This tissue was firm, resisting, though simple, of a whitish-gray colour, and contained no cavity. At its surface terminated vessels, which, when they reached this point, became suddenly impermeable.

Among eight children, in the bodies of whom Rilliet and Barthez found healed cavities, were four who died from general tuberculosis; four, again, appeared to have been cured of tubercular disease; three were destroyed by scarlet fever, and the fourth by chorea.

9. Some days back, I found in the body of a female, who died from gangrenous inflammation of the arm and purulent absorption, an interesting example of cicatrization of tubercle. In the apex of the upper lobe of the right lung was a furrow five centimetres long, which consisted of a fibrous cicatrix fifteen millimetres broad in the middle. A section of this part of the lung showed, in the centre of a considerable deposit of fibrinous substance and black colouring matter, a tubercular infiltration, the *corpuscula* of which

were still distinctly recognized under the microscope. There was nowhere any mineral substance. Here, therefore, the tubercular matter was not entirely absorbed, but was sufficiently isolated by newly-formed fibrous tissue.

We see, therefore, that the healing of tubercles, whether it is effected by calcareous transformation or by cicatrizing of cavities, is more frequent than is usually admitted.

4. *Upon the Expectoration from the* BRONCHI *in* TUBERCULOSIS.

It might be *a priori* believed, that between the expectoration of phthisical patients and the contents of cavernous ulcers of the lungs no remarkable difference ought to take place, seeing that the latter communicate directly with the *bronchi;* observation, however, has shown that the results are quite different from those inferred to take place. This is another proof that the positive sciences ought to have exact observation for their foundation.

We shall first advert to the individual elements which we have found in the expectoration of phthisical persons.

1. The first is mucus, which forms the basis of all forms of expectoration. Its fundamental characters are nowhere more strikingly delineated than by Professor Valentin in his recently-published manual of physiology.

We quote here his definition :—" By the name of *Mucus* we designate a gray tough substance, which is only miscible with water, but is not completely dissolved in it, easily attaches itself to solid bodies, and by stretching forms fibres, thereby protects and renders smooth the surface of corresponding parts, gives greater consistence to other fluids with which it is mixed, and when air is mechanically blended with it becomes frothy."

Mucus forms, as is known, the basis of all expectoration. Sometimes it is thick and scanty, and then the *sputa* form either a connected mass, or in completely-globular concretions lying close to each other; or it is mixed with much saliva and mucus of the mouth, and then forms a true serum for the mass of the *sputa*, each of which has as constituent a more or less viscid thickish mucus.

The first question which occurs in the examination of mucus is this: In what proportion are the mucus and *pus-corpuscula* mingled in tubercular expectoration? I have already expressed my opinion on this subject; and I refer for fuller information upon my views on this point in pathology to the work of Donné on Microscopic Inspection and its application to Medical Studies, and also to a criticism on the same work in the French *Annales de Chirurgie.* I here repeat only shortly, that I deny most positively the existence of peculiar mucous corpuscules, and that mucus in the normal state never contains those bodies which authors have described as mucus-*corpuscula*, very similar to *pus-corpuscula.*

A twofold error has been here committed. In mucus really not morbid, observers have erroneously taken for a peculiar formation, namely, mucous bodies, very new epithelial cells mixed with it, or the developed *nuclei* of old epithelial scales. But that epithelium is only mingled with mucus, and by no means constitutes an element of that secretion, we learn from the study of the development and reproduction of the epidermis. The second much more frequent error is, that often the observer has mistaken *pus-corpuscula* for *mucus-corpuscula;* and as their resemblance in imperfect observations must be acknowledged, so it has been attempted to establish between the two marks of distinction which are untenable. Many mucous membranes, namely, those of the nasal cavities, the larynx, the windpipe, the *bronchi*, that of the organs of generation also, in particular, very often, as soon as they are exposed to any irritating condition, secrete true *pus-corpuscula;* and of the identity of these with what have often been described as *mucus-corpuscula,* I have been convinced by many exact observations.

Doubtless this matter is mixed with mucus, effused from a free surface, and produces in this manner very little general disorder; and a slight catarrh passes over almost unnoticed, while a whitlœ indicates its presence by suffering and pain lasting for days. But in the one case the purulent matter is formed with the greatest facility, while in the other, confined by solid fibrous parts, it compresses and irritates delicate nervous fibres, sinews, and skin. The pus-globules are the same in both cases.

The products of an irritated mucous membrane have, therefore, been often mistaken for the elements of its normal secretion; and this mistake affects a circumstance not yet sufficiently appreciated in our present pathology, to which we shall often revert in our investigations; namely, that any given similar pathological process, according to the importance of the organ in which it takes place, may give rise to the greatest difference in symptoms and prognosis.

The much sought for tests of purulent matter, therefore, must be regarded henceforth as useless, since in the slightest affections of certain mucous membranes, *pus* and mucus are mixed with each other in different proportions. (4.)

2. Pus-Globules.—These form, as readily results from the preceding statements, the most uniform, and next to mucus the most abundant of the component parts of tubercular *sputa*, as well as of *sputa* generally. They possess, nevertheless, no diagnostic value, and by no means denote the presence of ulceration of the *bronchi* or of the pulmonic tissue, since they may take

place in considerable quantity in simple pulmonary catarrh, and in chronic *bronchitis* unconnected with tubercles. I observe here in general, that after I had often expected in the course of researches to arrive at an actual uniform diagnostic element in the *sputa* of phthisical patients, I must at last confess, and I believe I cannot do so too strongly, that the most accurate microscopical examination of *sputa* in *tuberculosis* has furnished no decided character. This point shall be considered afterwards.

In section the *pus*-globules in expectoration present all their characters; and the observer recognizes normally, with moderately high magnifying power of from three to four hundred diameters, the *nuclei* without having recourse to acetic acid. Usually he sees two *nuclei*, sometimes three, rarely four. A certain proportion of them contains one single *nucleus;* but these globules with one *nucleus* present neither a peculiar shape, *i. e.* exudation *corpuscula*, nor does this *nucleus* part into several; but the different number of *nuclei* forms only varieties.

Sometimes we find a moderate proportion of *pus*-globules, which have lost their round spherical shape, have become smaller, and only indistinctly show the *nucleus;* when treated, however, with acetic acid, they appear distinctly. These appear to be *pus-corpuscula* taken in the process of drying.

3. Epithelium fragments in different forms are not uncommonly found in tubercular expectoration. On one occasion I saw in the ward of Andral an instance. The physician had from auscultatory symptoms recognized phthisis in the first degree. In the expectoration was found almost no purulent matter, but a quantity of white flakes, which were entirely composed of membranous exfoliations of plaster-epithelium. The large *foliola* of plaster-epithelium proceed usually from the mucous membrane of the mouth. From the *bronchi* alone proceeds a sort of smaller granular plaster-epithelium, as also frequently cylinder-epithelium, and sometimes ciliated-epithelium. In one instance I found a distinct transition form between the first two sorts. (3.)

4. Proper tubercular substance I have never distinctly observed in the expectoration of the phthisical. I find in my observations one case in which I thought I saw it in moderate quantity, and one in which I saw it only doubtfully. The latter case, therefore, proves nothing; and against the first I must object, that this observation relates to a time when I had not yet directed the strictest attention to this point of my inquiry. In general it may be maintained, that the presence of actual tubercular *corpuscula* has not hitherto been proved in expectoration.

5. A fifth element is formed by a large amount of granular substance, which, according to what has been formerly adduced on the disintegration of tubercles, probably derives its origin from

them; this, however, continues hypothetical, since I find it in no characteristic form.

6. Frequently we find in tubercular expectoration a proportion of small yellow spots and pellicles, scarcely one millimetre in size, in which microscopic inspection proves an elastic consistence, and the stratified arrangement peculiar to fibrous concretions, as well as many granules, and not unfrequently *pus-corpuscula*, elements accordingly which present the greatest similitude to pseudo-membranous formations.

7. Sometimes we find in the expectoration distinct pulmonic fibres, which in that case appear to possess considerable diagnostic value. Their aspect is so peculiar, that they cannot be confounded with any other formations, particularly not with the fibres of the windpipe. As they can only appear in expectoration when the pulmonic tissue is partly destroyed, they denote distinctly the existence of a hollow ulcer of the lungs.

8. In very frequent cases we find fat in greater or less quantity, namely, adipose vesicles in expectoration.

9. Blood-globules are frequently found, as rupture of the capillaries as well as of the large vessels is by no means rare. In the last case are found *coagula*, enclosing at once fibrinous matter and red globules.

A large proportion of blood-globules are distributed among the mucus secreted by the *bronchi;* but I have not observed any peculiar changes among these globules.

10. Not unfrequently are found in expectoration aggregation-globules, which, however, present nothing remarkable.

11. Small INFUSORIA, particularly *vibriones*, are found accidentally only, in consequence of the uncleanliness of the expectoration vessels; and the observer, in examining, cannot be sufficiently on his guard, as we find in the *sputa* remains of food, muscle-cylinders, plant-cells, &c. Thus, the *sphæræ lenticulares* described by Gruby* as proper to tubercular matter, are nothing but starch-*corpuscula* accidentally mixed with the expectoration; and of this it is easy to be satisfied, if we compare his figures, fig. 89 to 95, with the delineation of starch or *fecula* given by Dujardin at p. 18.† It is impossible, therefore, to be sufficiently cautious, when we see that a microscopical observer so distinguished as Gruby has fallen into this mistake.

Lastly, we find occasionally in expectoration,—12, elements of colouring matter; 13, crystals, and 14, amorphous mineral granules. These particles are only exceptional.

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* Gruby, Observationes Microscopicæ ad Morphologiam Pathologicam. Vindobonæ, 1840, page 27.

† Dujardin Observateur au Microscope. Encyclopedie Rohret. Paris, 1843.

If we compare all these elements which we have found in the expectoration of phthisical persons, we find that the tubercular expectoration presents nowhere any characteristic element, that no means are hitherto known of distinguishing with certainty proper tubercular substance, and that the individual occurrence, taking place only exceptionally, for an important diagnostic element, is that of disintegrated pulmonic fibres.

My own observations, adduced independently, agree, however, in many respects, with those given in the judicious treatise of Bühlmann. This author, however, proceeds to an extreme, when he denies the want of one element in general proper to tubercle, however correct this may be as to expectoration.

A very large proportion of the expectoration in phthisis is positively furnished by the *bronchi*, and possesses no nearer relation to tubercles. The cough, the bronchitic symptoms, which, for the most part, attend the presence of pulmonary tubercles, and which, as the most important phenomena, the majority of practitioners endeavour to subdue, appear to me to have their chief cause in the wasting of one portion of capillary vessels by the development of tubercles; the blood has consequently to find a supplemental circulation, as Schroeder van der Kolk and Natalis Guillot have clearly proved. In this the bronchial arteries appear to play an important part, and hence arise excessive distension, true *hyperæmia* of the bronchial vessels, and often chronic inflammation. The occasional very copious discharge of mucus and purulent matter appears to me to be one of the modes by which the disturbed circulation endeavours to lighten itself of part of its contents, as it allows to exude through the bronchial capillaries the fluid part of the blood, which then is converted into mucus and purulent matter.

What becomes of the softening tubercle? Part of it becomes fluid, and is converted into a fine grained fluid, and in this form is excreted with the products of the bronchial secretion. But part of the content, it is highly probable, after it has become more or less fluid, is absorbed by the multiplied vessels surrounding the ulcerous cavity, and is thus conveyed out of the economy along with the different *excreta*. How, otherwise than by absorption, can we explain the almost complete disappearance of tubercular *corpuscula* in the calcareous transformation of crude or softened tubercles?

The difference, at first sight so great, between the content of ulcerous cavities and the matter of expectoration, is accordingly easily explained in this manner. Though it might even prove that this result did not find the directly useful application so much to be desired in different cases, yet it has a negative result positively expressed. By demonstrating the want of the diagnostic value of the expectoration in *phthisis*, it possesses the advantage of guarding against errors and erroneous conclusions.

5. *On the Integrity of the Lungs, in more or less general* TUBERCULOSIS.

Since Louis promulgated, in 1829, the law, that when tubercles appear in any one organ, the lungs are also affected, the correctness of this law has been impugned in many cases. The exceptions adduced depend partly on inaccurate observation, partly relate to children, to whom this law does not apply. Louis founded his observations on adults, and the law thence educed appears to be most valid after the age of fifteen years. The authentic cases which are at variance with this law, compared with the great number of cases in which it is correct, are so small in number, that, by the very few exceptions, they establish the generality of the law on an extended scale. Louis himself has, in the last edition of his work, not only repeated the confirmation of his law, as founded on experience lasting for fifteen years, but cites in all, only three credible cases of exception. As to children, Papavoine, and Rilliet and Barthez, have established different proportions from those derived from adults. The first has, among the bodies of fifty children affected with tubercles, found the lungs sound only in twelve instances, accordingly, about one fourth ($\frac{12}{50}$, $\frac{6}{25}$). Rilliet and Barthez noted, among three hundred and twelve bodies containing tubercles, forty-seven cases ($\frac{47}{312}$) in which the lungs contained no tubercles, consequently, about one seventh. The exceptions were very frequent between the ages of three and five years; something less frequent between one and two years; more rare between six and ten years.

This proportion my observations establish very decidedly. In adults I have as yet found only one single instance of an exception quite decided; and this exception relates to a case so interesting, that I think it necessary to communicate the observation. As to *tuberculosis* in children, I made part of my observations in hospitals similar to those of the above-mentioned authors, in the Paris Children's Hospital. I recorded, towards the end of the year 1842, numerical circumstances, particularly remarkable in this respect. From the 24th of October to the 31st December 1842, I had occasion to make forty-four inspections of bodies, eighteen instances of which were examples of generally diffused *tuberculosis;* equivalent, therefore, to something more than two-fifths ($\frac{18}{44} = \frac{9}{22} = \frac{3}{7}$). In these eighteen bodies I found three times, consequently, in one-sixth of the cases ($\frac{3}{18} = \frac{1}{6}$) tubercles in other organs without the lungs containing any; once in a child of two years, who presented tubercles in the bronchial glands, in the *peritoneum*, in the *ileum*, and the membranes of the brain; the second time in a child of two years and a-half, who presented tubercles in the cervical, bronchial, and inguinal glands; lastly, the third time in a child of three years, who had tubercles in the cerebellum, in the bronchial glands, and in the *ileum*.

That, however, the law announced by Louis, though not applicable in children to the lungs, is applicable to the bronchial glands, the researches of Rilliet and Barthez prove most decidedly.

Childhood has, when compared with adult age, presented to me the following differences in reference to *tuberculosis*:—1. Tuberculosis appears more frequently in the cerebral membranes in childhood than in adults. 2. The glandular system, and especially the bronchial glands, are more frequently affected than the superficial glands, namely, those of the neck. 3. Tubercles in the *peritoneum* are also much more frequent in children than in adults.

As to the greater frequency of *tuberculosis* in children, this cannot be extraordinary, when we remember that this disease, fatal in so large proportion, carries off many children in early life; consequently, those surviving childhood, numerically considered, must present a smaller number of tuberculous subjects.

In the districts, especially the parts of French Switzerland, where I reside, the Canton Waadt, the proportions are however entirely different; and, indeed, *tuberculosis* is there much less frequent among adults than among children.

The instance of tubercles of the *peritoneum* above-mentioned, without the appearance of these bodies in the lungs, is here subjoined.

I. K., forty-five years of age, of strong corporeal frame, had in youth suffered from various scrofulous disorders. Subsequently, after attaining manhood, he had frequent attacks of inflammatory diseases; for instance, *bronchitis*, frequent rheumatic pains, and, from time to time, rose of the face. In the spring of 1840, he was exposed to various chills, and without having any definitely characterized disease, he felt himself feeble and unwell till the beginning of August, at which time diarrhœa with abdominal pains, especially in the right side of the abdomen and in the region of the colon, came on. The diarrhœa, which at first caused from six to eight evacuations daily, was very obstinate. The abdomen was swelled, and felt doughy. Under the use of leeches applied over the pained parts, mild diet, and afterwards gentle astringents, the diarrhœa was abated. But, notwithstanding, that soon a substantial though light form of nourishment was prescribed, the strength failed, the patient, previously plump, wasted daily the pains in the belly continued, though in a form less violent; the doughy feeling of the abdomen was distinctly increased, without any definite swelling being recognized; the pulse was also accelerated, being 96 in the minute, and latterly in the evening so frequent as 108 beats.

Very profuse exhausting sweatings underwent a temporary cessation after the use of China-decoction. The urine was turbid during the whole course of the disease. The diarrhœa became more violent from time to time, but usually only one or two liquid stools took place in the space of twenty-four hours. After a slight improvement appeared in the month of January 1841, there ensued in February, after previous drawing pains in the lower extremities, inconsiderable œdema of these members, which continued to the last. In the alvine discharges microscopic examination proved the absence of any purulent matter, but the presence of crystals and longish striated leaflets, consisting probably of bile. At the close of February digestion began to be impaired; diarrhœa and abdominal pains were more intense. Sleep was interrupted, and the pulse was small and very quick. The use of food was followed by vomiting; during the last days the breathing was difficult; and, after progressively-increasing weakness, the patient expired on the 14th of March.

I must add that the patient, twelve years before the appearance of this disorder, had suffered violent pricking pains in the belly, which, however, apparently had no immediate consequence.

Inspection of the body thirty hours after death presented the following results.

The lungs, examined with the greatest care by my colleague, M. Le Docteur Bezancenet, who had latterly treated the patient in common with me, and by myself, presented nowhere the slightest trace of tubercles, and were at their posterior and inferior region the seat of hypostatic injection and redness. The heart was sound.

On opening the abdomen we found the whole *peritoneum* covered and filled with tubercles, varying in size from the head of a pin to a large hazel-nut, and everywhere between the same so large a quantity of black melanotic substance that the whole *peritoneum* had thereby acquired a black and yellowish speckled appearance. Some of these tubercles were softened internally; but the greater part were in the stage of crudity. In many spots were found around them distinct vascular injections, and the *peritoneum* was considerably thickened in spots, in some to the extent of one inch; it was readily lacerable, and where there were no tubercles, it was of jelly-like consistence.

The stomach presented in its large curvature a spot occupying the middle, which was intensely injected, and showed the mucous membrane softened, swelled, and containing black colouring matter. In the *ileum* appeared in several places small ulcerations, with vascular injection and swelling of the mucous membrane. More considerable ulcers, of the size of a Kreuzer piece with prominent edges, appeared in the *cæcum*. In the colon was a number of large ulcers, from four to twelve millimetres in diameter, at the bottom of which the muscular coat was for the most part destroyed. In the *ileum* was found one single submucous tubercle. The peritonæal covering of the liver and spleen, like all the other parts of the *peritoneum*, contained tubercles; yet both these organs were perfectly sound; the liver was very bloodless.

The left kidney was dark-red in colour, enlarged and softened, and presented the characters of chronic inflammation.

The other organs were in a state of perfect integrity.

As tubercles of the peritonæum rarely take place in adults without pulmonary tubercles, we may adduce here the most important points, which in similar cases may render the diagnosis certain.

1. LOCAL PHENOMENA.—Uniform swelling of the abdomen, feeling doughy without circumscribed swelling; wandering, yet not very violent, but almost constant pains in the belly, little aggravated by pressure.

2. GENERAL PHENOMENA.—Fever more or less continuous; the pulse above ninety beats in the minute, especially in the morning. Irregular alvine discharges, diarrhœa, tendency to colliquative sweats and sedimentous urine, emaciation, loss of strength, alteration of the complexion, but which consists rather in paleness than in the straw-yellow colouring of cancer. Œdema of the lower extremities, without askites, from the pressure of the tubercles on the vessels proceeding to the lower extremities.

6. *On the changes in the Pleura in* TUBERCULOSIS.

That the *pleura* becomes tuberculated as well as other organs, and may contain the several forms of tubercles, is by no means remarkable. But it is much more interesting to study the other changes, both in structure and function, which this serous membrane undergoes in the course of pulmonary tubercles.

The *pleura* often acquires in the lungs of phthisical persons a remarkable degree of thickness and vascularity, and I find in my observations cases noticed in which it had acquired the thickness of one inch. The membrane then consists, in the majority of instances, of indistinct layers of cell-fibres, which individually appear not rightly separated, and enclose a quantity of granular substance between them. The greatly-developed vessels may be traced in a remarkable manner from the surface of the lungs into the different parts of the *pleura*. Occasionally this vascular thickened *pleura* forms bridges, which unite the lobes of the lung with each other. In one of these bridges Schroeder has, if I mistake not, proved by injection the existence of lymphatic vessels. That this thickening of the *pleura* is effected partly by inflammation and by the organization of false membranes, is manifest. To me, however, it appears a mistake to believe that inflammation alone is the cause of this phenomenon. I am of opinion that in this instance an increase in nutrition at the same time takes place, and that the pleura becomes a true supplemental organ of circulation, conveying, therefore, many more vessels than in the normal state, and becomes the seat of a more intense deposition of its fibrous elements. By this means, at the same time, the lung which is becoming progressively thinner by the process of ulceration, is rendered more solid in the vicinity of cavities, and obtains a protecting, thick, fibrous investment, which, often with certainty, prevents perforation and pneumothorax.

I have here touched a very important point in the natural history of pulmonary tubercles, namely, the origin of a supplemental circulation through the vessels of the large circulation. This subject I shall elucidate more completely.

Schroeder van der Kolk, so highly honoured in physiology and pathology, has already, in his *Observationes Anatomico-Pathologicae et Practicæ Trajecti,* 1825, mentioned this supplemental circulation. (5.) In the dissertation of Lespinasse, above-mentioned at page 43, is the following passage, which expresses very distinctly, in few words, the discovery of the Dutch physician :—" Jam, uti probavit cl. Promotor, per vasa nova sanguis e pulmonibus in vasa intercostalia transit, quo fit ut circulationis campus dilatetur, atque ita sanguis e pulmonibus extrorsum derivetur."* "The blood, it has been proved by this distinguished inquirer, passes by new channels from the lungs into the intercostal arteries, by which arrangement the sphere of circulation is enlarged, and the blood is withdrawn from the lungs by derivation to the external parts."

This topic has been recently made the subject of express researches by a profound French physician, M. N. Guillot.† I refer for particular details to the writings of the authors; but I shall here present the most important points in their inquiry, arranging them in the following propositions :—

1. The capillary vessels of the pulmonary artery, are obliterated in the circumference of tubercles, and to greater extent the more abundant the tubercles are.

2. Around the tubercles is formed a vascular investment, in which are developed, independent of the general circulation, new vessels which afterwards establish a connection with the bronchial and

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* Lespinasse. Specimen anatomico-Pathologicum de vasis raris pseudomembranarum tam arteriis et venosis, quam lymphaticis. Rhen. Trajecti, 1842.

† L'Experience.

intercostal arteries. This communication is further favoured by new and independent vessels which are formed in the false membrane and surface of the *pleura*. These vessels do not penetrate the crude tubercles, but constitute a serpentine vascular mass at the inner surface of cavities.

3. The matter of injections, thrown into the aorta, is again found in the pulmonary and bronchial veins, and in the *vena azygos*.

4. While, therefore, the tuberculated pulmonic tissue possesses vessels by the bronchial and intercostal arteries, a communication is formed between the circulation in the aorta, and that in the lungs; and, by the mixture of venous and arterial blood, this fluid is conveyed, in a state of progressively increased impurity, into the different organs.

We have already mentioned, that by the tubercular deposition a number of vessels are compressed; these are, accordingly, partly obliterated, while partly thereby other capillaries are inordinately filled, and hence arises part of the phenomena of hyperemia and inflammation in *phthisis*. But the question whether the new vessels described by Guillot, independently of the general circulation, are really formed round tubercles, and in the pleura, I have subjected to a very accurate inquiry, as the labours of Guillot were well known to me during my researches.

I have, however, never observed anything similar. On the one side embryological investigations, on the other pathological researches have taught us, that in the higher vertebrated animals, the new vessels are always formed in a centrifugal direction from the main circulation. For more specific details on this point, I refer, as to the formation of blood-vessels in the embryo, to the joint researches of Dr Prevost and myself.

As to the pathological new formation of vessels in false membranes and *neoplasma*, especially tubercles and cancer, I have ascertained partly by injections, partly by many exact anatomical researches, that independent vessels are never formed in these productions, but that they invariably receive their blood from vessels without the circulation. I have often been at first deceived, and this I think has happened to many inquirers, in taking, namely, streaks and accidental ramifications of blood-colouring matter for new vessels. In the lungs I have uniformly found the small capillaries both surrounding the tubercles, and in cavities, under the microscope, in full and direct communication with the large neighbouring vessels. The vessels of the *pleura*, and particularly its organized false membranes, invariably proceeded from the surface of the lungs, whenever no adhesion had taken place. That by narrow adhesion with the walls of the chest, afterwards vessels might be traced from the intercostal arteries, into the pleura and its false membranes, and thus anastomosed directly with the vessels proceeding from the pulmonary artery, is further not extraordinary.

The other statements of Guillot I have not myself been able to verify by adequate proper observations; but I see in the sincere and upright scientific character of the extract given in the *Experience*, and in that of the author, a security for their credibility; and these observations I consider as some of the most valuable contributions by pathology to the history of tubercles.

The *pleura* appears to me to perform a twofold part, when thickened it surrounds the diseased pulmonic tissue. On the one hand, it protects the lung from laceration; and on the other, it acts as a receptacle for a part of the blood which the lungs are no longer in a condition to receive, and by communication with the intercostal arteries as a true supplemental circulation.

We shall, however, quickly see, that the pleura does not always accomplish this protecting part of its duty, but often by fluid and purulent effusion, more or less abundant, accelerates the catastrophe in phthisis. I shall not repeat the well-known facts in the history of pleurisy attending pulmonary *tuberculosis*; but I subjoin a short sketch of several rarely observed facts which I find in my record of observations.

1. In one case, in which, in phthisis, in the last stage, pneumothorax with pleurisy and considerable effusion, had taken place; the fluid presented the following abnormal properties. It was in appearance thin fluid; and in order to determine the proportion between serum and cells, I measured the height of both in a glass-tube. Upon thirty-eight millimetres in height, thirty-one were occupied by serum, and seven by globules; furnishing therefore about $\frac{2}{9}$ ($\frac{7}{31}$). The serum was turbid, yellowish, inclining to green; it contained suspended a little fat. The globules were 0.01 parts of a millimetre in diameter, were pale, flattened, and contained in their substance some very small granules, transparent in the centre, but no *nuclei*, and consequently no real pus-globules. After rest and precipitation by nitric acid, it was possible to recognize in the fluid a large quantity of albumen.

2. In another case, in the body of a man of twenty-three years, who having been long affected with tubercles, died in the Hotel Dieu, in consequence of amputation, we found within the chest, besides pulmonary tubercles, a considerable quantity of fluid effused within the pleura, which thickish, scarcely fluid, yellow, inclining to red, examined by the naked eye, looked almost like tubercular matter taken in the process of softening. This substance resembled very much the concrete purulent matter which is found in congestive or cold abscesses, in consequence of caries of the *vertebrae* in the praevertebral sacs, filled with thick secre-

tion, no longer fluid. By the microscope this semi-fluid pleuritic effusion showed the presence of a quantity of puriform globules of 0.01 parts of a millimetre in diameter, like those in the previous case, without proper *nucleus*, and besides, many true *pus-corpuscula*, and a large quantity of *pus-corpuscula* disintegrated and dissolved into a fine-grained fluid. I found nowhere any trace of tubercular elements. This, therefore, forms an excellent instance of the value of microscopic diagnosis between softened tubercle and concrete purulent matter.

3. On the 6th January 1843, I found in Paris, in the dead ward of the Hopital de la Pitié, the following remarkable alteration in the pleura, which was left further almost unrecognizable. I learnt, when I attempted to obtain correct information on the case, that it was the body of a man of forty years, who had suffered for four months under chronic pleurisy, with tubercular complication, and who had died within a few days of acute *meningitis*.

The false membranes of the pleura were the seat of a formation appearing to be cartilaginous and bony, in other spots fibrous, in others jelly-like, and in several places tubercular. These different formations were seated between the fourth and ninth ribs of the left side. The ribs were in their bony substance entire. The *periosteum* was easily detached from them. The costal *pleura* was one centimetre thick, and was in several places, but not everywhere, adherent to the pulmonic *pleura*. The substance of this mass formed from the pleura, and the transformed pseudo-membranes, presented a very thick fibrous structure, which, where it was most compressed, had the milk-white appearance of cartilage, of which, nevertheless, no trace was found in its substance. The apparently ossified matter contained only amorphous mineral substances, accumulated in various places, but nowhere any true osseous elements. The jelly-like matter—which was contained in many spots in moderate quantity, in excavated spots of the false membrane and the thickened pleura, and resembled closely animal jelly—consisted of a network of fibres, which enclosed a fine granular transparent mass.

In many places these different formations presented a degree of redness, which arose partly from vascularity, partly from mere imbibition of blood. In many places tubercles were found infiltrated between these different parts in more or less considerable amount. These showed distinctly their characteristic *corpuscula*.

This is one of those cases which, without the aid of minute microscopical examination, might easily lead to the most erroneous views upon the conversion of false membranes into cartilage and bone—views which have not hitherto been subjected to a sufficiently rigorous criticism.

I terminate here my observations on tubercles of the lungs, on which I have still much to communicate. But I confine myself to the limits of the present essay.

NOTES.

(1.) The statement, that the tubercular process is regarded by persons of the highest authority as a modification of suppuration, must be received with considerable deductions. Few pathologists deserving the name have, for the last twenty-five years, in this country, entertained any view of the kind; and all the best informed physicians have regarded the disintegration of tubercles as a peculiar process totally different from suppuration. In 1822, indeed, at the time when Dr Abercrombie published his Observations on Diseases of the Lungs, he considered the breaking down of tubercles, or rather their subsequent effects, as of the nature of ulceration. Laennec first described the process as one of softening of a peculiar kind; and repeatedly in this Journal, since that time, we have spoken and reasoned on the softening of tubercular masses as a process totally different from that of suppuration.

Schroeder, in his Anatomico-Pathological Observations, published in 1826, gave the opinion that the process differed from suppuration by the absence of the effusion of coagulable lymph, by new granulations not being formed, and by no new vessels being formed, and consequently, by the tubercular mass being softened by a species of partial death.*

Dr Carsewell, though he does not express himself very distinctly, yet seems to understand that the softening process differs from suppuration. "When the softening process," he says, "takes place in tuberculous matter, it is clear, from what I have just stated, that it cannot be owing to any change originating in this morbid product. Besides, when speaking of the consistence of this matter, I showed that in many organs it is always in a state of fluidity, and consequently does not require to undergo the change in question supposed to be necessary to, or at least to facilitate, its expulsion. When the tuberculous matter has become firm, owing to the circumstances which I have already explained, it may at some future period be converted into a granular-looking pulp, or pale grumous fluid of various colours, from the admixture of serosity, pus, blood, &c. which have been effused or secreted by the tissues subjected to its irritating influence. The pus and serosity pervade the substance of the tuberculous matter, loosen and detach it. These changes are further promoted by atrophy, ulceration, or mortification of the surrounding or enclosed tissues, the blood-vessels of which have been compressed or obliterated by the tuberculous matter."†

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* Observationes Anatomico-Pathologici et Practici Argumenti. Auctore J. L. C. Schroeder van der Kolk. Fasciculus I. Amstelodami, 1826, p. 98.

† Pathological Anatomy. Illustrations, &c. London, 1838. Tubercle.

Here, therefore, the process of softening is ascribed to admixture of pus, serum, or blood, secreted by the adjoining tissues in a state of irritation from the presence of the tubercular bodies; and Dr Carsewell accordingly represents the softening-process as most commonly commencing around or at the circumference of firm tuberculous matter.

The following passage, relating to the same subject, is from a work published in 1840:—

" The manner in which tubercular masses are softened or broken down and discharged, or what may be termed the mechanism of tubercular softening and excavation, has attracted some notice, and deserves consideration. At one time, it was imagined to be either identical with, or analogous to, suppuration in other tissues; and it was supposed that tubercular *vomicæ* were merely abscesses of the lungs. But the process, though perhaps analogous to, is not the same with suppuration. It seems to be more complicated, and not so uniform in its progress. It seems to be difficult to ascertain at what part softening commences. In one case it may begin in the centre, and proceed to the circumference; in another it may begin at the circumference, and go round the whole mass, detaching it from the surrounding lung; in a third case it may begin at once at the centre, and at the margins; and, in other cases, it has been observed to commence at the same time in several parts of the substance of the tubercular mass. The latter is the course especially in the case of large tubercular masses. Cerutti,* who entertains this opinion, states that, in the section of a tubercular mass in this state, the portion or spots about to be softened appear to lose firmness and to become friable, and if examined by the microscope, it presents numerous minute holes, as if punctured by a needle. This condition extends over the whole mass, until its parts are detached from each other; and minute grains are found amidst a semifluid or fluid opaque mass. While this is proceeding, a communication is established with one or more bronchial tubes, as already stated above, the small ends of which are destroyed or dissolved in the softening process, and the semifluid matter reaching them irritates them, causing secondary catarrh, and excites coughing, by which it is expelled. The transition of this semifluid matter through the bronchial tubes is the cause of the redness and villous appearance of the mucous membrane of the bronchial tubes, so generally observed in the lungs of those destroyed by this distemper.

"On the means by which this softening is effected, different opinions have been entertained. An opinion very generally received is, that the tubercular masses, acting in some manner as foreign bodies, give rise to irritation and vascular action in their vicinity, and in this manner induce a sort of congestive and inflammatory afflux of fluids, in which they are melted or dissolved in an imperfect suppuration. This opinion is chiefly supported by those facts which show that tubercles begin to soften near the circumference of the masses.

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* Collectanea quaedam de Phthisi Pulmonum tuberculosa scripsit et in Universitate Lipsiae, in die xviii. Junii A. C. 1839 publice defendit, Dr Frid. Petrus Ludovicus Cerutti, Pathologiae et Therapiae Specialis, P. P. O. Des. Lipsiae, 1839, 4to, p. 22.

"Many tubercular masses, nevertheless, seem to possess an internal or innate tendency to destruction. Their texture is imperfect; and in some instances the internal substance begins to soften, apparently whether any irritation of the surrounding lung has taken place or not. There is no doubt that, in a considerable proportion of cases, the presence of the irritation of severe *bronchitis* or peripneumony appears to have pushed the tubercular masses into speedy liquefaction; and the frequency with which the symptoms of pneumonic inflammation are succeeded by those of consumption, shows that in the formation of softening at least, if not in the development of tubercular deposits, inflammatory congestion has great influence."[*]

These facts are sufficient to show, that the idea, that tubercular softening is a process of suppuration, has been long exploded among accurate observers in this country. Suppuration may doubtless take place as an effect of tubercular softening, either in the substance of the lungs, or in the bronchial mucous membrane. But in this instance it is a secondary process, altogether subordinate to that of the disintegration of tubercles.

Everything that we observe relating to these bodies shows, that their softening is a process of disintegration,—that is, solution and separation of component particles, beginning within the tubercle, and depending on its intimate structure, if such a deposit can be said to possess structure. This softening does not always commence in the circumference. On the contrary, it usually begins in the centre. But an analogous process of imperfect suppuration commences at the same time, or soon after, around the tubercle; and hence, apparently, the mistake both as to the nature of the process and the situation in which it first takes place.

(2.) The want of distinction here adverted to by M. Lebert arises from the vague, confused, and indiscriminate manner in which the terms tubercle and tubercular are employed. Tubercle is employed to designate the following objects:—1st, the tubercular, or rather tyromatous matter deposited within the air-cells of the lungs; 2d, the aggregated, irregular, nodulated masses formed in the lungs by the union or aggregation of multiplied small tubercles; 3d, tubercles of the serous membranes; 4th, kirrhosis of the liver; 5th, the white enkephalomatous masses formed within the liver and at its surface; 6th, the hard tubercles found in glandular and cutaneous cancer; 7th, the chronic enlargement of the cutaneous follicles in akne and molluskum; and 8th, certain tumours forming in the course of nervous chords.

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[*] Elements of Practice of Medicine, by David Craigie, M. D. Vol. ii. p. 963. Edinburgh, 1837 and 1840.

Now it is to be observed, that in some of these objects it is intended to denote a substance or secreted product of particular chemical and pathological characters; in others it is employed to designate the irregular and nodulated form of the growth; and in others it is meant to convey the idea of a mere small hard swelling. The first case is that of the tyromatous or tubercular matter deposited in the lungs, the serous membranes, and the mucous membranes. Tubercle of the liver is meant to designate chiefly *kirrhosis*, or the irregular tuberculated surface caused by the indurated enlargement of the *akini* of that organ; and the term has no reference to chemical characters. Again, when it is applied to the enkephalomatous tumours, it is employed to designate the irregular nodulated appearance which the tumours give the organ. Thirdly, it is very common to speak of tuberculated *sarkoma* and tuberculated skirrhus, and in this instance the term is evidently intended to express the irregularly-nodulated form and hard consistence of the swellings.

With the view of obviating this confusion, the writer of these Notes proposed in 1828 the term *Tyroma* to designate scrofulous tubercle of the glands, lung, and other organs,* as one which marked only its chemical and pathological characters, and reserving tubercle to denote the hard irregular swellings occurring in skirrhus. The change has not been generally adopted, and the confusion continues.

Skirrhus or cancer often appears in the form of hard nodules, varying in size, and the tumour may then be called tuberculated; but the term is liable to cause confusion and misunderstanding. Perhaps a preferable denomination might be *nodulated*.

(3.) Epithelium is the name given to the internal epidermis, or thin pellicle, covering all the free surfaces of the mucous membranes to their most remote recesses, and even, according to Henle, the walls of shut cavities. It is so denominated to distinguish it from external scarf-skin, epidermis, or cuticle. At the same time the structure is so similar, that various of the organic characters of both are given conjointly.

As the different forms of epithelium have been distinguished by Henle into three orders, according to the nature of their constituent cells, it is necessary to give a short notice of the characters of these cells, as ascertained by microscopic examination.

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* Elements of General and Pathological Anatomy. By D. Craigie, M.D. London and Edinburgh, 1828, chap. xiii., sect. ii., p. 457; chap. xxii., sect. ii., p. 729; chap. xxiii., sect. ii., p. 797; and Second Edition, 1848, pp. 222, 337, 674, and 737.

The simplest element of the epithelium are cells with a *nucleus*, which neither in form nor in chemical composition everywhere and at all times resemble each other. The most common nucleus is roundish or oval, from 0.002 to 0.003 parts of one Paris inch in diameter, more or less flat, for the most part colourless, but sometimes coloured pale-reddish like blood-globules, normally distinguished with one or two small punctiform nucleated *corpuscula*, which have a diameter varying from 0.0002 to 0.0008 parts of one Paris inch. Besides these, through their dark outline appear granules irregularly disseminated, finer and paler granules in alternating numbers in the *nucleus*. Not unfrequently the edge of these granules is remarkably dark as if knobbed, and there is found then inside these bodies a second concentric but clearer circle, so that the whole object has the appearance of a disk, with elevated boundary-edge. The nucleus is insoluble in vinegar, in caustic ammonia, and carbonate of ammonia, but is dissolved in caustic and carbonated potass.

The cell is for the most part clear as water and colourless, though often dotted with minute points. Whether it is hollow and filled with fluid, and consequently a true cell, or a solid globule, it is not easy to determine by inspection. If it were hollow, and the cell-wall sufficiently thick, the observer ought to distinguish the outline of the latter in the form of two concentric circles, the interval between which would be represented by the thickness of the wall. This, however, is not the case; and we must infer, that either no cavity is formed in the interior, or the cell-membrane is so delicate that it appears only as a mere line. The last is, according to analogy, the most probable; and in recent or young cells it receives illustration by bursting the wall, on which a clear fluid is effused, and occasionally a nucleus escapes. When the cell is roundish and sufficiently large, we see that the nucleus is eccentric and lies on the wall itself. In flat cells it usually projects on both sides. The size and shape of the cells varies much. Sometimes its outer circumference surrounds closely like a concentric circle the cell-nucleus; sometimes it intersects its diameter six or seven times.

By the form of the cells Henle distinguishes three different kinds of epithelium; the plaster-epithelium, the cylinder-epithelium, and the ciliated epithelium.*

1. PLASTER-EPITHELIUM.—The cell repeats in general the outline of the *nucleus*, as it is more or less large; and accordingly, either lies close to the *nucleus*, or forms around it a spacious vesicle. To the epithelium, composed of cells of this kind, Henle applies the name of PLASTER-EPITHELIUM. It is the most widely distributed form, and the only one which, by a peculiar

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* Allgemeine Anatomie. Lehre von der Mischungs-und-Formbestandtheilen des Menschlichen Körpers. Von J. Henle. Leipzig, 1841, 8vo. Seite, 222–223.

chemical change in the cells and accumulation of several layers, possesses that degree of thickness and compactness which renders it capable of performing the part of a protecting covering. This might be denominated LAMINAR-EPITHELIUM.

2. CYLINDER-EPITHELIUM.—The cells have a cylindrical or conical shape, with apices directed to the mucous membrane; hence are placed as fibres close to each other. The *nucleus* lies for the most part midway between the basis and the apex of the globule. Cells thus constituted form the CYLINDER-EPITHELIUM.

3. The CILIATED-EPITHELIUM consists of quite similar cylindrical or conical-shaped cells, the elements of which are distinguished by *cilia*, or separate projecting processes which they carry at their free broad extremities, and which possess the property of gently waving motion.

In certain situations these different forms appear to pass into each other;—a circumstance which has given occasion to admit a TRANSITION-EPITHELIUM.

Plaster-epithelium, besides forming the cutaneous *epidermis*, is found in the mouth, nasal cavities, larynx, windpipe, and bronchial tubes, to near their small ends. Here it seems to disappear, and is replaced by cylinder-epithelium. Some observers, nevertheless, call in question the existence of true-celled epithelium in the air-cells.

Cylinder-epithelium is also found in the alimentary canal beyond the cardia and through the *ileum*, in the ducts of the liver and gall-bladder, and pancreatic duct. It extends over the whole intestinal tube to its inferior outlet at the anus in the human body; and there it stops. It is found also lining the generative organs of the male.

Cylinder-epithelium is a mere modification of plaster epithelium.

Ciliated-epithelium is found in the bronchial tubes, in the nasal cavities, and those of the different sinuses in the lacrymal passages, and in the Eustachian tube. It is also found in the mucous membrane of the reproductive organs of the female, from the *cervix uteri*, through the body of the uterus, and into the Fallopian tubes. It is found likewise in the ventricles of the brain.

(4.) The doctrine that the purulent or puriform expectoration in phthisis proceeds not from the tubercles but from the inflamed bronchial membranes, is not new. It has been recognized more or less generally in this country for a long period. The author, however, has the merit of adding the testimony of microscopical evidence to that already adduced.

It is long since all practical physicians have given up the distinctions between *pus* and mucus, as means of forming a diagnosis. It is nevertheless important to observe, that the expectoration in phthisis presents certain characters so unequivocal, that the practised eye never mistakes the source of that expectoration.

(5.) The substance of the passage containing the account given by Schroeder of this change in the pulmonic vascular system, is already given in the account of the doctrines of that author in the forty-fifth volume of this Journal, pp. 529 and 530, Edinburgh, 1836.

Art. V.—*Historical and Statistical Sketch of the Progress of Epidemic Fever in Glasgow during the year* 1847. By R. S. Orr, M.D., lately Superintendent of the Royal Infirmary, Glasgow.

All are aware that fever has prevailed, not only in Glasgow, but also generally throughout the country during the past year, to an extent which has been truly fearful, and which is well calculated to awaken the greatest anxiety in the minds of those who have escaped its ravages. All possess some general knowledge of a few of the leading facts connected with this subject, but few are acquainted with the minutiæ. They may have read the newspaper notices, and glanced at the figures and statements there exhibited; and there also they may have observed from time to time recorded the deaths from "Fever," from "Typhus," from "Malignant Fever," of those in the upper and middle ranks with whose names they may be familiar, and of none more frequently than the hard-worked and often ill-remunerated members of our own profession. They can, however, form no idea of the hundreds of the poor and destitute who have, during the past year, paid the last penalty to the epidemic visitor which has so recently decimated their numbers, and reduced many of the survivors to a state of the greatest poverty and helplessness. They can form but a small estimate of the labours of those who have been employed in procuring the necessary accommodation for the thousands afflicted with the malady, and of the fatigues of those who have watched over and attended to their numerous wants.

It is with the view of attempting a short sketch of the history and progress of the epidemic of the last year that I now write. Doubtless there are others more competent to the undertaking. But as the position I so recently held in the Royal Infirmary of Glasgow gave me many opportunities of watching the advance of the epidemic during the greater part of its progress; and as, in the way of duty, my attention was constantly called to the subject, and directed to the means which were employed for staying its course, or alleviating the sufferings of those who felt its ravages,—I trust that those who take the trouble to read the following remarks, will believe, that they are dictated by a wish to furnish correct data from which some knowledge of the extent and prevalence of the late epidemic may be arrived at, and also a desire to exhibit the means which have been used to stay its progress. My information, gleaned from the experience of the infirmary, and from the very able and valuable report of the managers, drawn up by Professor Thomson, has been verified by my own observations; and the numerical data on which it is founded, were, for the most part, compiled from the records of the institution, which during the greater part of the year I kept, and of which my successor in office, Mr Steele, to whom I am indebted for much valuable information, has since had the charge. For the statistical details and information from the other hospitals and charities, I am indebted to the officials of each, who had the best opportunities of noting the transactions of their respective institutions, and on whose information therefore may be placed full reliance.

In the following sketch I have adhered strictly to the returns furnished from the different hospitals, the district surgeons, and other authentic sources, and have purposely refrained from any conjectures as to the numbers of cases which may have occurred, and of which no return has been made, although, doubtless, these must have amounted to a considerable number. I shall endeavour to investigate, as fully as my data will permit, the progress and amount of epidemic fever which prevailed during the year 1847 in the city and suburbs of Glasgow, and also show the means which were employed to meet and combat its effects.

In the summer of 1845, fever was at the lowest point of declination in Glasgow which had been known for many years. The immunity which the city then enjoyed from this malady, which is there generally so prevalent, was matter of great thankfulness to its citizens, many of whom were as much surprised at that period to be told that their fever wards contained, on more occasions than one, only eight patients, as they have been last year when they learnt, that as many hundreds were often accommodated within their precincts. The satisfactory state of the public health at the time above mentioned is specially alluded to by the managers of the Infirmary in their Report for the year 1845, where they particularly call attention to the very small number of fever cases treated—"a number less by several hundreds than it had on any previous occasion, since the opening of the present fever hospital in 1829, fallen to their lot to report."

In tracing the history of fever from this period, when it may be said to have been at its minimum, I shall begin by analysing the data furnished by the statistics and experience of the Royal Infirmary. From these it appears that the present epidemic (for although on the decrease, it cannot yet be said to have terminated) commenced about the end of the autumn of 1846, some months previous to its appearance in Edinburgh, and went on steadily increasing till the summer of last year, when it attained its maximum. To illustrate this point, and in continuation of a table I published last year,* showing the progress of fever and its regularity in advancing and retiring, it may be mentioned, that the admissions to the fever wards of the Infirmary amounted in autumn 1846, when the disease began to be severe, to 627; in winter to 942; in the spring of 1847 to 1205; in summer, when it was at its greatest height, to no fewer than 1729; in autumn to 1248, and in winter to 967; showing at first a manifest and steady increase, and latterly as steady a decline.

The total number of cases of fever treated to a termination in the fever wards and auxiliary hospitals under the jurisdiction of the managers of the Infirmary, during 1847, amounted to the very large number of 5244. Of these 4420 were dismissed cured, and 804 died,—exhibiting a mortality of 15.39 per cent. or two deaths in every thirteen cases.

On only one occasion have the cases treated in the fever hospital amounted to anything like the above number; and this occurred, exactly ten years previously, in 1837; when they reached the somewhat higher number of 5387; and on only two previous occasions did the mortality reach the magnitude of 15 per cent., namely, in 1836 and 1839, when it was in the latter 16½ per cent. and in the former nearly 16 per cent.

In consequence of the great number of applicants for admission to the fever wards, these, at the very commencement of the year, were so full, that the managers of the Infirmary were on many occasions compelled to refuse admission. About the end of January, they succeeded in opening in the neighbourhood of the Infirmary a new hospital, which had recently been erected as a lock-hospital, and which afforded accommodation to about seventy female patients. By the middle of February this also was quite full; the applicants for admission were more numerous than before; and the refusals steadily increased in number. At this time, I was requested by the managers to keep an account of the number of patients who applied for admission, but who, from the want of accommodation, were refused. This record was kept daily during the greater part of the epidemic, and when at its height, there were frequently as many as one hundred or one hundred and twenty cases on the list of applicants whose claims it had been impossible to overtake; while, at the same time, I have reason to know, that many were debarred from applying, in consequence of knowing the utter hopelessness of such an appeal.

In anticipation of future epidemics, the managers of the Infirmary had taken steps for the erection of an auxiliary hospital for the treatment of fever, on a piece of ground which they had formerly purchased, and which seemed well adapted for such a purpose, when they were met by the strongest opposition from the neighbouring proprietors. This opposition they did not think it expedient to withstand, and having advantageously disposed of the ground, they purchased a large new building in Bridgeton,—a district where a fever hospital is much required. This building they considered as very advantageously situated, and as capable, with very little alteration and outlay of expense, of being easily adapted to the purposes of a fever hospital. Here again, however, they were met by opposition; one of the principal proprietors in the neighbourhood having applied to the sheriff for an interdict to prevent them from executing their intended purpose. The managers, in the latter instance, resolved on defending their rights, and instructed their law-agent accordingly; it being apparent, that unless they made a stand here, they might be driven from one quarter of the city to another without getting a site for their hospital. A final decision has not yet been given, and the case is still *sub judice;* but in the words of the report of the managers, where an account of these occurrences is given, "the decision is one destined to have a most important influence on the arrangements to be made in future for dealing with fever epidemics."* The schemes of the managers for the public benefit being twice thwarted in the manner above adverted to, they resolved, although with considerable reluctance, on the erection of a temporary wooden shed within their own grounds, which was accordingly done; and on the 24th of June it was opened for the reception of fever cases. This erection was capable of containing about 140 patients. Above 80 cases were admitted during the two first days, and in a very short period all the beds were occupied. The additional labour and fatigue, both of body and mind, which these arrangements imposed on the hospital staff of the Infirmary, can only be fully appreciated by those who have held similar positions, and performed similar duties. Too much praise cannot be awarded to the managers of the Infirmary for the very spirited and energetic manner in which they met and endeavoured to combat the ravages of the epidemic. The calamity was a crying one, and as such it required that extraordinary measures should be adopted to grapple with it.

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* Edin. Med. and Surg. Journal, No. 171, April 1846.

It is greatly to be regretted, that the proposed auxiliary hospital had not been erected when first the ground was purchased for this purpose, so that it might have been ready for all future outbreaks of disease. The erection of a permanent auxiliary hospital seems to be a perfectly legitimate method of appropriating a portion of the funds of the Infirmary, because the sum so invested in a permanent building, remains as it were funded capital; and, in times of epidemic disease, the managers would be justified in looking to their fellow-citizens for increased contributions to enable them to meet the pressing emergencies of the times, and defray the expenses of such an additional establishment. But, on the other hand, it appears very questionable how far it is incumbent on the managers of the Infirmary to provide temporary accommodation in the shape of sheds, &c., for the overplus of fever cases which their ordinary accommodation will not receive; and the same remark applies equally in the case of the Edinburgh Infirmary. Such temporary accommodation is the most expensive method in the long run of meeting the evil, and it is generally badly adapted for the purpose, especially in the winter season. Besides this, it cannot be supposed that the Infirmary boards are to grapple with such a disease as epidemic fever, unaided or unassisted. The funds at their disposal are raised for the maintenance of the institution, and, so far as they will go, for the alleviation of epidemic disease, but not for the erection, and support of temporary buildings, which are comparatively useless after the immediate purpose, for which they were erected, is served. In addition to this, it may here be mentioned, that the Infirmary lies under permanent obligations to a number of country parishes, which long ago subscribed to its funds, which have thus acquired a right to recommend patients, and from which no return is now obtained, but whose interests the board are bound to attend to. The building a permanent auxiliary fever hospital seems, then, a proper disposal of a portion of the Infirmary funds, because, as already stated, there it stands a sum invested for the public good. It can easily be conceived that the expending a large sum of money on extensive temporary erections, every time that epidemic fever visits a city like Glasgow, would be a most extravagant waste of money. But, admitting, on the other hand, that this measure was necessary, which, failing all other suitable accommodation, no one at all conversant with the subject will for a moment doubt to have been particularly the case last year, it appears very far from clear, that it was either the duty or the province of the managers of the Infirmary to provide it. A conviction of the truth of this seems to have made them very unwilling to erect the shed on their grounds; knowing, also, as they did, and as the sequel will fully prove, that they could not, even with such accommodation, afford admission to all who would apply.

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* Since the above was printed, a very decided judgment in favour of the managers has been pronounced by the sheriff-substitute, and, on appeal, confirmed by the sheriff-depute.

In the act 8th and 9th of Victoria, cap. lxxxiii., for the amendment and better administration of the laws relating to the relief of the poor in Scotland, (4th August 1845,) it clearly appears to be declared the duty of the parochial boards of the different parishes to afford medical relief to the poor of their districts. In the 69th clause of that act, it is thus enacted, "That in every parish or combination, it shall and may be lawful for the parochial board, and they are hereby required, out of the funds raised for the relief of the poor, to provide for medicines, medical attendance, nutritious diet, cordials, and clothing for such poor in such manner and in such extent as may seem equitable and expedient." This is a very comprehensive clause, and invests them with great power for the relief of the sick and diseased poor, and indeed requires this at their hands. Now, after the ordinary accommodation for epidemic disease has been filled up, does it not clearly appear to be the duty of the parochial boards to furnish the additional accommodation for the poor of their respective districts, and thus afford the only "relief" which is calculated to have any affect in checking the malady? There is nothing in the act which prevents their erecting and maintaining such accommodation for the treatment of epidemic disease; all that is necessary being, as in the case of the erection of poor-houses, the submitting their plans to the approval of the Board of Supervision. I am aware, that, in a previous clause of the act to the one above quoted, it is enacted, that it is lawful for the parochial boards to subscribe from the poor's funds to any public infirmary, dispensary, &c., such sums as may seem to them reasonable and expedient. This is quite right, and is generally sufficient in times when disease is not more prevalent than ordinary; but, when the contrary is the case, it seems distinctly to be their province to provide the necessary accommodation for the sick and diseased. It is true the Infirmary board have lately, as on former occasions, erected fever sheds and temporary accommodation for those afflicted with epidemic disease, because, before the enactment of the recent poor law, they seemed the only parties to undertake the duty. But now that the law imposes the care and guardianship of the sick and diseased poor, as well as of the healthy, on the parochial boards, the furnishing proper accommodation for them in times of distress when disease is unusually prevalent, appears a very reasonable and proper application of their funds. The justice of these observations seems, after some little delay, to have been acknowledged by the parochial boards themselves, at least in the case of Glasgow, in their ultimately furnishing such accommodation, as will be presently more fully shown in prosecuting the history of the epidemic. In the case of Edinburgh, the task seems to have devolved entirely on those in the direction of the Royal Infirmary. Let any one read the last report of the managers of the Edinburgh Infirmary, and he will find this fact fully confirmed, as well as the apathy of the parochial boards much complained of. No doubt these boards have had their hands full during the past year, which was one of much disease and unexampled distress. The working of the new poor-law system also is yet far from being matured, and the delay that has in many cases occurred, it is to be hoped, is to be accounted for on these grounds. The labours and fatigues of the inspectors of poor, and of the officials connected with the Town's Hospital of Glasgow, have during the past year been perfectly unprecedented; these, I have often been told, being frequently protracted to a very late hour.

Having thus gleaned from the experience of the Infirmary so much of the history of the epidemic, and examined the extent to which it has been made subservient in combating the disease, I now proceed to notice briefly the returns made to the Town's Hospital authorities by their district surgeons. These gentlemen are seventeen in number; and it is their duty to afford medical relief to the poor of the City Parish of Glasgow. They have made regular returns of the numbers of fever cases visited by them in each of their districts since the year 1832, and a weekly record is kept of old cases, (that is, I presume, of those formerly reported as under treatment,) new cases, and cases sent to the Infirmary. By the kindness of Mr Hill, who keeps this record, which, it is much to be desired, should be regularly continued, I have been allowed access to it, and have been enabled from it to construct the following table, which shows the weekly *average* number of fever cases reported, as above-mentioned. In examining this table it is particularly necessary to bear in mind that it is the *average* weekly number for each year that is there exhibited, and that the numbers sent to the Infirmary are excluded.

TABLE *showing the average weekly number of Fever Cases visited by the City District Surgeons from* 1833 *to* 1847.

Year.	Old Cases.	New Cases.	Total.
1833	12	13	25
1834	16	18	34
1835	10	11	21
1836	21	26	47
1837	50	63	113
1838	10	15	25
1839	3	7	10
1840	13	26	39
1841	15	27	42
1842	7	13	20
1843	392	287	679
1844	60	35	95
1845	10	7	17
1846	38	34	72
1847	230	209	439

It may excite surprise in the minds of those unacquainted with the real cause, to observe from the above table, that there was a much greater amount of fever in 1843 than in 1847; and more especially will this be the case, when they remember that no additional accommodation, beyond what was provided by the ordinary fever hospital, was called into requisition. But the explanation is to be found *first* in the nature of the fever which was then prevalent. It was a new fever which had not been seen before;—the relapsing fever, as it was called, on account of the almost uniform occurrence of relapse. It was a mild disease, the mortality being only about 4 per cent.; and ran its course much more rapidly than the epidemic now prevalent. According to the mortality bills of the city of Glasgow for the year 1843, drawn up by the late Dr Alexander Watt, the total number of deaths from fever in the city and suburbs for that year amounted to 1398. Taking the mortality as above stated at 4 per cent., this number would indicate about 35,000 as the number of individuals attacked by the disease. The fever of 1847 was, on the other hand, of a much more malignant type, and, as has already been seen, its mortality was very high. I have ascertained from the registers of the above mentioned lamented statist, that the total deaths from fever in 1847 amounted to 4346; but as the mortality of the present epidemic has been 15 per cent., that number indicates only 29,000 as the number attacked. A *second* explanation of the apparent disproportion between the prevalence of fever and the amount of hospital accommodation,—and it is a strong one,—is the fact that there was incomparably less destitution in 1843 than in 1847. By far the larger number of those affected underwent the disease at their own homes; indeed it appears from estimates made at the time,* that only about a tenth part of those affected were sent to hospital. During 1847 there has been the greatest depression and stagnation in trade, with numerous and very extensive failures; and poverty and destitution, caused chiefly by the great influx of Irish paupers, have prevailed to a most fearful and calamitous extent. The fever hospitals were in consequence crowded to overflowing with houseless wanderers; and the number of those marked "houseless" in the Infirmary registers under the head "residence," exceeded all parallel. In many cases the *residence* given was scarcely worthy the name, and the tenants would with more propriety have been termed *lodgers* than *residents*. Many poor, starved, destitute, and diseased creatures were brought and laid down before the doors of the Infirmary, their relatives, if they had any, not knowing what to do with them; and, in numerous instances, it was destitution and starvation more than fever which was their chief affliction. Numbers also were sent to hospital in such a weak state, that, as appears from a very accurate table kept for me by Mr Brown, the janitor of the Royal Infirmary, of the deaths from all diseases, including fever, occurring in the Infirmary during last year, 133 died within 36 hours after their admission, of whom 107 died within 24 hours, and 50 within 12 hours. In 74 of the deaths in the fever wards the patients had been less than 36 hours in hospital, and of these 38 died within 24 hours, and 16 within 12 hours after admission.

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* See Dr Perry's Observations on the Sanatory State of Glasgow in 1843, Edin. Med. and Surg. Journal for July 1844. Page 87.

To destitution, therefore, are we principally to look for the cause which, during the past year, has filled our fever hospitals to overflowing. Hundreds of strangers have undergone the disease in them, who might truly be said to have had no other place where to lay their heads; and many were compelled to seek hospital relief who in ordinary times would have weathered the storm without such aid. The actual number of cases of fever reported by the district surgeons of the city parish in the epidemic of 1843, from the month of May till the end of the year—a period of eight months—is stated by the late Dr Perry (and his estimate is not exaggerated, but the contrary) to have been, in round numbers, 14,000; whereas in 1847 they cannot, in that parish, for the whole year, be estimated at more than 11,000; and yet, the hospital accommodation required in 1847 has been more than five times what was necessary in 1843. The highest number of cases reported weekly in the district surgeons' returns amounted in 1847 to 319, while in 1843 they were frequently above 700, and on one occasion reached the number of 855.

Up till the month of July there was no additional accommodation furnished for the reception of fever patients beyond what has already been mentioned as having been afforded by the managers of the Royal Infirmary. Meantime the epidemic advanced steadily. Some weeks previous to this, however, the parochial board of the City Parish, seeing the fearful strides which the disease was taking, the great ravages it was committing among their poor and destitute, and the complete impossibility of all of them being accommodated in the Infirmary, resolved on having an hospital of their own. They were fortunate enough at this time to secure the old Town's Hospital in Clyde Street for the purpose, though a short time previously they, in conjunction with the managers of the Infirmary, as stated in their report, had failed in obtaining it for a fever hospital. On taking possession of this building, which had been for some time unoccupied, several destitute families were found to have taken up their abode there, for want of any other residence; and as several of the individuals were labouring under fever, it was necessary to remove them to the Infirmary, in order that the alterations necessary for converting the establishment into fever wards might be proceeded with. These arrangements were sufficiently completed to enable the board to open a part of the establishment on the 7th of July. On that day between seventy and eighty fever patients were admitted, and the accommodation, which ultimately numbered 634 beds, was afterwards, as soon as ready, filled up. The following numerical statements, taken from the records of the institution, and for which I am indebted to the kindness of Mr Hill, will exhibit the amount of benefit which this charity has conferred on the community.

There were admitted, from the period of its opening (the 7th of July) till the end of the year 1847, 4433 patients; of these 3937 were treated to a termination, 3377 having been dismissed, recovered, and 560 having died, and 496 remained under treatment on the 31st of December.

In consequence of the great number of cases of fever which existed in the very large and wide spread Parish of the Barony, of the total inability of the managers of the Infirmary to receive them into their fever hospital, and of the conviction that their being removed from their own abodes to hospital was the only means of staying the epidemic, the Barony parochial board resolved on the erection of a temporary building for their accommodation. This building, which was of wood, they were allowed to erect on a piece of ground in Anderston, belonging to Mr Lumsden, late Lord Provost of Glasgow, who liberally granted them the use of it. It is situated in a district where fever was very prevalent, and where it was consequently much required. There happened to be a large dwelling-house and offices previously on the ground, which were found well suited for the resident hospital staff, for culinary purposes, &c. Wooden sheds were erected which at first contained 150 beds; subsequently, accommodation for 100 additional beds was procured, so that the total number amounted to 250. On the 5th of August this establishment was opened, and from that date till the end of the year, the number of cases of fever admitted was 1806; of these 1601 were treated to a termination, 1414 having been dismissed recovered, and 187 having died, and 205 remained under treatment on the 31st of December. For these statistical data I am indebted to my former assistant in the Infirmary, Mr Paterson, who is now connected with this establishment.

On reviewing and summing up the statistical data collected from the records of the three hospitals, of the transactions of which the preceding remarks are a brief sketch, it will be found that the total number of beds appropriated for the reception of fever cases, when the epidemic was at its height, amounted to 1254. The total number of fever patients who have found refuge in the hospitals of Glasgow during the year 1847 has amounted to no fewer than 11,425; of these 10,762 have been treated to a termination, of whom 9211 were dismissed recovered, and 1551 died; and 907 remained under treatment on the 31st of December 1847. These results will, however, be more clearly exhibited in a tabular form, which I therefore subjoin.

TABLE *showing the number of Fever Cases treated, &c. in the Fever Hospitals of Glasgow during the year* 1847.

	Admissions.	Dismissions.	Deaths.	No. remaining 31st Dec. 1847.	Mort. per cent.
Fever wards of Infirmary,	5186	4420	804	206	15.39
City Parish Hospital,	4433	3377	560	496	14.22
Barony Parish Hospital,	1806	1414	187	205	11.67
	11425	9211	1551	907	14.41

The mortality of the disease, as calculated from these data, amounted to 14.41 per cent., or one death in every seven persons attacked. The greater mortality of the cases treated in the Infirmary is accounted for by its having been larger in the first half year than in the second; but as the other hospitals were not occupied during the former period, no comparison can be made. It further appears from the above table, that the disease was, at the close of the year, far from being subdued, there being at that time no fewer than 907 patients in the three fever hospitals. Of these it is to be remarked, that 496, more than the half, were under the charge of the parochial authorities of the City Parish, and about a fourth under that of the Barony Parish authorities, showing the urgent necessity for the accommodation which these public bodies have latterly so fully furnished. Dr Alison, in his Treatise on the Management of the Poor,* relates the case of an Irish widow with four children, who was refused relief from the Charity Workhouse. The consequence of this was, that she and her children lived for some time in extreme destitution, in a close cellar, in a small but crowded close. One of the children took fever, the others soon sickened, and within a few weeks the disease spread to fifteen of the neighbours. He adds, that he has witnessed hundreds of such instances, and remarks, that if this family had been taken into the workhouse, or supported in comfort out of it, he firmly believes this little epidemic would never have occurred. The same reasoning applies to the subject before us. Who can calculate the number of similar little epidemics which the timely removal of the first case to hospital has been the means of checking; and how much more fearfully would the disease have raged and spread through the city had not the additional accommodation of 1024 beds been provided for the poor sufferers?

This seems the proper place to inquire who have been the chief sufferers from the recent epidemic. I now therefore proceed to show, from data derived from the sources from which the information already given has been obtained, that the Irish paupers, who, during the past year, have flocked in such numbers to Edinburgh and Glasgow, have been the principal victims of the disease, and that it is owing to their great influx and destitution that the epidemic attained such a degree of prevalence. The following table exhibits a statement of the numbers belonging to each nation of those treated in the Infirmary fever wards and in the Barony Fever Hospital during the year 1847; but as no record of this nature was kept in the City Parish Hospital prior to the 10th of September, the numbers for that establishment can only be given from that date till the end of the year.

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* On the Management of the Poor in Scotland, by William P. Alison, M. D., F. R. S. E. Edin. 1840. Page 192.

TABLE *showing the number of Scotch, Irish, English, &c. treated in the Fever Hospitals of Glasgow during the year* 1847.

	Scotch.		Irish.		English.		Foreigners.	Unknown.
	No.	Per cent.	No.	Per cent.	No.	Per cent.	No.	No.
Fever Wards of Infirmary,	2094	=40.08	3106	=59.45	15	=0.28	9	...
City Parish Hospital from 10th September, .	896	=39.64	1326	=58.67	38	=1.68	...	...
Barony Parish Hospital,	890	=49.28	884	=48.89	25	=1.38	2	5
	3880	=41.76	5316	=57.22	78	=0.83	11	5

This table shows, that of the above numbers of patients, considerably more than one-half were natives of Ireland, who, strictly speaking, have no legal claims on the parochial authorities for such an amount of medical relief. It certainly does appear hard, that a city like Glasgow, already sufficiently burdened with the poor of its own country, should be compelled, it may be said in self-defence, to afford relief to such a multitude of strangers. In many cases, after the Infirmary fever wards were completely full, it was often extremely painful to be obliged to refuse admission when those applying for it belonged to one's native country, knowing, at the same time, how many beds originally intended for their use were filled by the natives of another. The results obtained from an examination of the above table, show that the number of Scotch who were treated could quite well have been accommodated in the fever wards of the Royal Infirmary, without any additional accommodation. I have elsewhere[*] given a tabular statement of the numbers of the natives of Ireland who for several years back have received the benefits of treatment in the Infirmary fever wards. From that table it appears that during the last eight or ten years they have increased from 25 and 30 per cent. to 47 per cent. of the whole number treated. Last year, however, they will be found to have reached nearly 60 per cent., while the natives of Scotland amount to only 40 per cent. of those treated. Here, then, there appears to have been a complete change as regards the numbers treated belonging to each country, the Irish having now taken the place in our hospitals which the Scotch occupied ten years ago, and *vice versa;* and this frequently to the exclusion of the latter. These circumstances, however, speak very highly for the liberality and charity of the citizens of Glasgow, who have thus, by means of their hospitals, furnished such a great amount of relief to the distressed sufferers from the sister island.

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[*] Edin. Med. and Surg. Journal for April 1846.

It has been supposed* that the present epidemic is an imported disease, and this chiefly because of the immense number of Irish who have suffered from its ravages. To this opinion I do not feel inclined to subscribe. I readily admit that a very large number of its victims have been imported, and this appears to me to be the more correct way of viewing the subject. Hundreds of the poor distressed Irish who last year flocked to Glasgow and the other large towns, were, from their very circumstances, in a state which rendered them peculiarly liable to be attacked by fever. They were in utter destitution and poverty. Many of them half-starved; they came from their own country in this state; many of them from the rural districts, and were located, on their very first arrival, in the dense, overcrowded, ill-ventilated lodging-houses of Glasgow, from which, it is well known, fever is seldom absent. There, before their constitutions became accommodated to such an atmosphere, they met the epidemic, and themselves furnished the *materiel* on which it has committed such ravages, instead of, as has been supposed, communicating it to those they found there. Another argument in favour of this view of the subject is derived from the fact of the regular periodical appearance of epidemic fever. This has, by the most accurate observers, been remarked to recur every ten years in the city of Glasgow; and we have records of its occurrence in 1817, 1818, and 1819; in 1826, 1827, and 1828; in 1836, 1837, and 1838; and now we have it prevailing in 1846–1847–1848. This periodical prevalence of fever seems therefore totally independent of any importation of the disease, which has raged as extensively, as regards actual numbers, among the Scotch residents as formerly, although the great influx of Irish to the hospitals has, in calculations on the subject, tended to diminish the per centage of the former.

I have already hinted at the labours and fatigues of those who, during the past year, have been engaged in attending on the sick. The following particulars will show the very hazardous nature of the employment of these individuals.

In the City Parish seven out of the seventeen district surgeons took fever, of whom three died. In the Barony Parish, of the seven district surgeons three died of it; I have not been able to ascertain the total number attacked. In Govan Parish, one district surgeon had the disease, but recovered. In the Royal Infirmary, Mr Steele informs me, four hospital clerks had fever, of whom one died; and thirty-six individuals, including nurses, servants, and two matrons (of whom one died), had the disease, and of this number five died; the total number of servants, &c. employed in the fever department of the Infirmary being about forty-five. Of the Town's Hospital officials, eleven, including inspectors of poor, &c., had fever, of whom five died. In the City Parish Fever Hospital, two medical officers had fever, and one died; thirty nurses and servants also were attacked, and of these six died,—there being altogether about sixty employed. In the Barony Parish Fever Hospital one clerk had the disease and recovered, and thirteen nurses and servants, of whom one died. This establishment employs about thirty servants, &c. Two Inspectors of the Poor in the Barony Parish died of the disease; but I have been unable to ascertain the number attacked. Four officials connected with the management of the poor in Govan Parish were attacked with fever, of whom one died, and three connected with the Gorbals Parish, and of these two died.

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* Report on the Mortality of Edinburgh and Leith, by James Stark, M. D. Edin. Med. and Surg. Journal, July 1847, pp. 260, 261.

No fewer, therefore, than 117 persons employed in the treatment and care of the sick have been ascertained to have been affected with the epidemic, and of this number 30 have died: so that, while the disease has cut off one in every seven among the poor afflicted by it, it has been fatal to one in every four of those in attendance upon the sick who were attacked, and nearly one-half of the medical officers attacked have died of it. The following table exhibits these results.

TABLE *showing the number of Medical Officers and Officials of the different Charities in Glasgow who suffered from Fever in the year* 1847.

		Medical Officers			Officials.			Totals.		
		No.	Cured.	Died.	No.	Cured.	Died.	No.	Cured.	Died.
City Parish	District Surgeons	7	4	3	..	...	..	7	4	3
	Town's Hospital	...	...	...	11	6	5	11	6	5
	Clyde St. Hospital	2	1	1	30	24	6	32	25	7
Barony Parish	District Surgeons	3	..	3	2		2	5	...	5
	Fever Hospital	1	1	...	13	12	1	14	13	1
Govan Parish		1	1	...	4	3	1	5	4	1
Gorbals Parish		...	...	...	3	1	2	3	1	2
Infirmary Fever Wards		4	3	1	36	31	5	40	34	6
	Total,	18	10	8	99	77	22	117	87	30

I shall only in conclusion advert to two indications having reference to future outbreaks of disease, which seem to flow from the experience of the present epidemic; and which it appears very important should be attended to. The first of these regards the erection of district fever hospitals in, or rather near, the localities of the city and suburbs where fever is most prevalent. The propriety of this was long ago urged by the late Dr Cowan, and it is particularly alluded to by the managers of the Infirmary in their last report. What is 230 beds, which is the ordinary accommodation of the fever hospital, for a city like Glasgow, with its population of 344,200? We have seen that 1250 beds have scarcely been sufficient on this last occasion. Permanent, not temporary fever hospitals, should therefore be got ready as soon as possible, so that, when an epidemic appears, they may be made immediately available for the treatment of the disease, instead of having to be sought for or erected, while, in the meantime, the disease makes progress without any check being given to it. The managers of the Infirmary may be said to have already pledged themselves to have an auxiliary hospital without loss of time. Let the parochial authorities of the City and Barony parishes, and I may add, those of Govan parish also, each erect a similar establishment for the relief of their own poor in times of epidemic disease, and I will venture to predict, that by thus separating the sick from the healthy, and making an early impression on the disease, they will deal with the next epidemic in a much more economical and efficient manner than they have done lately, and save themselves not only from expense, but also from great labour and anxiety.

The second indication to which I would advert is equally important. It refers to the necessity of having the houses, apartments, and bedding of those removed to the fever hospitals thoroughly cleansed and purified. This appears a self-evident proposition. The removal of fever patients to hospital will have little effect in checking the disease, if the very beds in which they lay are the same night to be occupied by another set of *victims*, as they may be called; and these too, it may be presumed, often very unsuspecting victims. At the commencement of the present epidemic, and indeed after it had raged for a considerable time, little or no attention was paid to this point; and, there can be little doubt, the disease spread much more, in consequence of this neglect, than it would have done, had the very important measure I am now advocating been adopted. Lately, however, it is satisfactory to think, that it has been attended to, and that there are now individuals employed, whose duty it is to white-wash and fumigate the houses of those infected with fever, and wash and purify their bedding. It would be well, if each of the fever hospitals had a regular staff of white-washers and fumigators attached to it, to whom the addresses of those brought to the hospital should be communicated as soon as they arrive, in order that they might at once proceed and commence their work of cleansing and purification.

These means of removal and separation of the sick, and purification of their abodes, may be considered the most efficient measures for checking the prevalence of epidemic disease, and for preventing, so far as human means can do so, a recurrence of the distresses so recently experienced.

Dunoon, *February* 1848.

ART. VI.—*Abstract of the Return of the Mortality in* 117 *Districts of England for the quarter ending* 31*st December* 1847. Published by authority of the Registrar-General. Annual Series VI. 1847, No. 4.

THE quarterly returns are obtained from 117 districts, subdivided into 582 sub-districts. 36 districts are in the metropolis, and the remaining 81 comprise, with some agricultural districts, the principal towns and cities of England. The population was 6,612,800 in 1841.

57,925 deaths were registered in the last quarter. The average number of deaths, deduced from the returns of the corresponding quarter of nine preceding years, and corrected for increase of population, is 46,549. There is consequently an excess of 11,376 deaths in the quarter. The deaths registered in the December quarters of 1845, 1846, and 1847, are 39,291, 53,093, 57,925; the mortality in the first is to that of the last quarter nearly as two to three. The mortality was below the average in the autumn quarters of the five years, 1841–5, and above the average in the five years 1838–40, 1846–7.

A slight increase in the mortality was noted in the returns of the June quarter, 1846; the mortality in the following hot summer, when the potato crop failed, was excessive; cholera and diarrhœa were epidemic. In the autumn of 1846, as well as the winter and spring quarters of 1847, the mortality was still higher; scurvy prevailed in the beginning of the year, but in the summer the public health appeared to be slightly improved. Epidemics of typhus and influenza however set in, and made the mortality in the last quarter of 1847 higher than in any quarter of any year since the new system of registration commenced.

The deaths in the year 1845 were 166,000; in 1847, 215,000. The excess in 1847 is 49,000; or not less than 35,000 over the corrected average of 1839–45. The districts from which the quarterly table is made up have hitherto returned less than half the deaths in England; but it is not probable that the country districts have suffered to the same extent as most of those comprehended in the return.

In London, the deaths registered in the quarters ending December 1845–6–7, were 11,838, 13,221, and 18,553 (13 weeks).

The greatest number registered in any quarter of the nine previous years was 14,686 in the severe winter of 1845.

It was shown in the last quarterly report on the state of the public health, that if the chance that a child in the country under 15 years of age will die in three months be represented by 1, the chance that a child under 15 in London will die in the same time is represented by 2. It was also shown that the chance of dying among men above 35 in London, is to that in the country as 3 to 2; and it was remarked that "if the chance of dying is increased from 2 in the country to 3 in London, the liability to suffer from epidemics is raised still more." The truth of this proposition has unfortunately been too soon exemplified. The population was inadequately supplied with potatoes, and scurvy was prevalent in the beginning of the year. Meat and bread were dear; distress was rife; vagrants flocked in from the country, the poor Irish came to their kindred, the workhouses were crowded. In April and May typhus became epidemic; instead of the average of 34, 50 died weekly; it steadily spread and burnt on, until it killed 111 victims in a single week. Diarrhœa, dysentery, and cholera had been a little more fatal than usual throughout the year; 17, however, only died of these diseases in the first week of July; the mean temperature of the air was above 60°; the number of deaths rose to 38, 47, 67, 125, 128, 188, by the middle of August, and then gradually subsided. Notwithstanding the continued prevalence of typhus and scarlatina, the deaths in the last week of October were only 945; one person died of influenza, 36 of bronchitis (inflammation of the air tubes), and 62 of pneumonia (inflammation of the substance of the lungs). In the three weeks following, ending 20th November, the total deaths were 1052, 1098, and 1086; of which two, four, and four were by influenza; 49, 58, and 61 by bronchitis; 68, 79, and 95 by pneumonia.

The wind had generally been blowing south-south-west and south-west since the first week of October; the weather was unusually warm; a brilliant aurora was observed, and shook the magnets on October 24: it appeared eight times during the quarter; on Tuesday, November 16, there was a remarkable darkness; the wind changed to north-west, and amidst the various changes still blew from the north over Greenwich, at the rate of 160 and 250 miles a-day. The mean temperature of the air suddenly fell from 11° above to 10° below the average; on Monday it was 54°, Friday, 32°; the air on Friday night was 27°; the earth was frozen; the wind was calm three days, and on Saturday evening a dense fog lay over the Thames and London for the space of five hours. No electricity stirred in the air during the week. All was still,—as if nature held her breath at the sight of the destroyer come forth to sacrifice her children.

On Sunday the sky was overcast, the air damp, the wind changed in the night to south by east, and passed for four days over Greenwich at the rate of 200 and 300 miles daily; the temperature suddenly rose and remained from 2° to 9° above the average through the week ending November 27, when the deaths of 1677 persons, 819 males and 858 females, were registered; 771 persons under 15 years of age, 518 aged 15 to 60, and 388 of the age of 60 and upwards. Influenza was epidemic. On the first week of December, 2454 persons died; 1141 were males, 1313 females, 1012 children; 712 persons in the prime of life, 730 of the age of 60 and upwards. On the week following, 2416 persons died; 1175 males, 1241 females; 1016 under the age of 15; 698 at the age of 15–60, and 702 at the age of 60 and upwards. The deaths in the weeks ending Saturday, December 18, December 25, and January 1, were 1946, 1247, and 1599. 11,339 persons died in six weeks, and altogether the epidemic carried off more than 5000 souls over and above the mortality of the season. The epidemic attained the greatest intensity in the second week of its course; raged with nearly equal violence throughout the third week; declined in the fourth, and then partly subsided; but the temperature falling, the mortality remained high, not only through December, but through the month of January.

The epidemic was most fatal to adults and to the aged. Thus, in the three weeks ending November 13, the deaths under 15 years of age were 1553; in the three weeks of the epidemic ending December 18, the deaths under the age of 15 were 2846. In the same two periods the deaths at the ages 15–60 were 966 and 1970; at the age of 60 and upwards, 576 and 1999. The mortality in childhood was raised 83 per cent., in manhood 104 per cent., in old age 247 per cent. From the age of 4 to 25, however, the mortality was comparatively not very much increased; at the age of 10 to 15, the healthiest period of life, it was scarcely increased at all—in girls.

During the seven years 1838–44 the deaths of males in London were more numerous than those of females, in the proportion of 1746 males to 1674 females; in the second week of the influenza epidemic the proportions were reversed, for 1141 males and 1313 females died; in the six weeks ending January 1, the deaths of 5580 males and 5759 females were registered. Looking, however, at particular ages, the deaths in the six weeks under five years of age were—2321 males, 2009 females; from 5 to 55, males 1580, females 1507; 55 and upwards, 1678 males, 2,241 females. At all ages there are more females than males living in London; at 55 and upwards the males in 1841 were 71,384, the females 90,328; at 75 and upwards, males living 6788, females 11,147. A disease much more deadly in the old than in the middle aged and young people, therefore necessarily increases the total deaths of females more than the total deaths of males, without for that reason being more fatal to the female than to the male sex. The difference in the mortality of males and females from the epidemic is but slight, and can only be determined by nice calculation, into which I shall not enter here.

Influenza attacked those labouring under all sorts of diseases, as well as the healthy. The vital force was extinguished in old age and chronic diseases. The poison, permeating the whole system, fastens chiefly on the mucous membrane lining the sinuses of the face and head, and the air tubes of the lungs. Hence it is fatal to the asthmatic. The deaths directly ascribed to asthma in October and November, were 12 weekly; in the six weeks of the influenza epidemic, 77, 86, 78, 52, 14, 26, besides the numerous cases classed under influenza. 36 deaths were ascribed to bronchitis in the week ending October 30th, and 49, 58, 61, 196, 343, 299, 234, 107, and 138 in the nine following weeks. 62 deaths were ascribed to pneumonia in the same week, and 68, 79, 95, 170, 306, 294, 189, 131, and 148 in the nine weeks following. In some of these cases the inflammation specified was the primary disease, in others secondary, and in many it was purely influenza—mis-reported. There is a strong disposition among some English practitioners, not only to localize disease, but to see nothing but a local disease; hence, although it is certain that the high mortality upon record was the immediate result of the epidemic of influenza, the deaths referred to that cause are only 1157: namely, in the first week of November, 2, and in the eight weeks following, 4, 4, 36, 198, 374, 270, 142, 127; and these include nearly all the cases in which influenza was returned, whether as primary, or secondary in conjunction with other diseases. A similar defect has hitherto been found in the returns of all great epidemics. In 1665, the great plague year, 97,306 burials were returned in the London bills of mortality, only 68,596 of which were ascribed to plague. Influenza attacked persons labouring under other zymotic diseases; thus, the deaths from hooping cough rose from 12 and 25 to 65 and 71 during the epidemic; the deaths from measles rose from 43 to 96, 89, 69, 75, during the first four weeks of the epidemic, and then subsided to 37 and 58. Typhus, which had been fatal to 70 and 80 weekly, rose to 132, 136, and 131, in the second, third, and fourth weeks of the influenza epidemic, and then fell to 83 and 74. Although influenza is not mentioned in these cases, it is in others; and there can be little doubt that two or more zymotic processes do often go on simultaneously in the blood and body; a fact of profound interest to the pathologist, and worthy of attentive investigation.

The epidemic was much more fatal in some districts of London than in others. To show this, I take the deaths in each of the

London districts during the six weeks from November 21, 1847, to January 1, 1848; and comparing them with the population, obtain the relative mortality. It was at the rate of 46 per annum to 1000 living in London; the rate of mortality in the seven years, 1838–44, was at the rate of 25 annually to 1000; the mortality was consequently raised for six weeks by the epidemic about 80 per cent. above the average. Lewisham, including Blackheath, Sydenham, and Eltham, is one of the healthiest districts in London; the ordinary rate of mortality is 17 annually, during the epidemic it was 27. St George-in-the-East is one of the unhealthiest districts; the ordinary rate of mortality is 29 in 1000, the rate of mortality during the epidemic was 73. The increase in Lewisham was 10; in St George-in-the-East, 44. The latter district suffered four times as much from influenza as the former.

The epidemic of influenza killed twice as many people in the insalubrious parts of London as it did in those less unhealthy; its fatality in Lewisham and St George-in-the-East was, as we have seen, one to four. The annual average rate of mortality for London, in 1730–39, was 41 in 1000; the rate in the six weeks of the epidemic of 1733 was 72 in 1000; the increase was 31 in 1733; the increase in 1847 was 21.

Let us look to the few country and small town districts in the present return. The deaths in St Albans (population 17,000 in 1841) during the last quarters of the four years 1844–7, were 94, 75, 91, and 76. The registrar of the sub-district of St Albans says, "No epidemic has visited this district." The deaths in the Kendal district (population 35,000) during the last quarters of the four years were 160, 213, 268, and 155; in Anglesea (population 38,000) the deaths were 155, 163, 206, 158. The registrar of Llanddausaint, Anglesea, says, "latterly influenza has been prevalent;" but it was, as has been seen, not very fatal. In the Isle of Wight (population 43,000) the deaths in the December quarter of the four last years were 235, 167, 201, 179. The registrar of the Godshill sub-district says, "The district would be healthy but for influenza, which has just made its appearance; but no case has yet been fatal." Epidemic influenza is mentioned in the notes by the registrars of Maidstone, Brighton, Portsea Island, Northampton, Cambridge, Norwich (mild—"fatal to few"), Yarmouth, Exeter, St Thomas (this is the district round Exeter; influenza had just made its appearance at Topsham), Plymouth (very fatal in St Andrew's sub-district, deaths in October 48, November 49, December 103; Charles the Martyr sub-district, deaths in October 17, November 26, December 63), Redruth, Penzance, Bath (Lansdown sub-district, in the three months, 16, 21, 47; the Abbey, 18, 19, 58), Bristol, Clifton, Stroud, Hereford city ("influenza is just appearing"), Shrewsbury, Walsall, Wolverhampton, Burslem, Birmingham, Aston, Coventry, Leicester, Lincoln, Nottingham (St Ann subdistrict, deaths in three months, 35, 33, 76), Basford, Derby, Stockport, Macclesfield, Great Broughton with Chester, Liverpool, West Derby, Preston ("influenza prevailed during the two last weeks of December"), Bury, Bolton, Wigan, Prescot, Chorlton, Manchester (Ancoats, deaths in October 169, November 135, December 270); Salford, Ashton, and Oldham Sheffield (West sub-district, deaths in October 27, November 27, December 85); Huddersfield, Halifax, Bradford (Horton subdistrict, "no particular disease, except in ten days, commencing November 27, when influenza was prevalent"), Leeds, Hunslet, York (Walmsgate sub-district, deaths, October 61, November 52, December 99), Sunderland, Tynemouth, Newcastle-upon-Tyne, Carlisle ("no epidemic disease prevalent at present in Dalston, Carlisle"), Pontypool, Wrexham, Anglesea. The quarterly return includes few of the country districts, but it is evident that influenza pervaded England generally; in many places it appeared later than in London; some places it has not yet reached at all, or its visitation has been so slight as not to attract attention. The mortality was raised in the unhealthiest towns; but on the whole much less in the mixed town and country districts than in London. The deaths in the December quarters of the four years 1844–7 were, in the districts of London, 13,819, 11,838, 13,221, 18,553 (13 weeks); in the other districts, 30,261, 27,453, 39,872, and 38,320. In the latter districts, notwithstanding the epidemic influenza, the mortality on the whole was lower in 1847 than in 1846. The weather, as will be seen from Mr Glaisher's lucid statement (given at page 30 of the Report), did not differ much in any part of the country.

Edinburgh.—Dr Stark, to whom Edinburgh is indebted for tables of mortality, states that in that city influenza attacked great masses of the population twice during the course of November; first on the 18th, and again on the 28th day of the month. It appeared in both cases during keen frost, and an excessively damp, thick fog, which came on rather suddenly after a few days of very mild weather. The total deaths returned, exclusive of still-born, were in October, 521; November, 728; December, 1001. The mean temperature of Edinburgh in December was 39°; the highest, 57°; the lowest, 21°.*

Our knowledge of the progress of the epidemic in other countries is necessarily imperfect, as no weekly tables are yet published in any of the great continental cities. We learn, however, from the medical and other journals that the *grippe*, which had prevailed for a week, was at its height in Paris about December 4, when one-fourth or one-half of the population was laid up ("Un quart, si non la moitié de la population est couchée."—*(Gazette Médicale.)* It is stated that 50,000 of the inhabitants of Madrid were in bed suffering from influenza on the 11th January. The epidemic still prevailed on the 19th, and was exceedingly fatal. London was probably attacked a few days before Paris; Madrid a month later. In the former epidemic (1782), influenza attacked London at the end of May, France in June, Italy in July, and Spain in August. It travels faster now. The present epidemic reigned in Rennes on 30th October (Professor Toulmache, in *Gazette Médicale*). Influenza attacked the crew of the Lousquor French packet in the Mediterranean in October, and was so general that in difficult circumstances the surgeon believes the vessel must have been lost. The epidemic did not prevail in Marseilles when the Lousquor left. The Albatross and Nile had cases at sea, but the epidemic was in full force at Toulon and Marseilles when they left port.* Dr Laval, a member of the council of health at Constantinople, states that influenza broke out in that city towards the end of August 1847, and prevailed, though not to a very great extent, for a month or six weeks.† A slight epidemic of cholera broke out in October, and still reigns in Constantinople. Respecting the influenza epidemic in Germany, Russia, and Italy, no authentic information has come to hand.

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* Report by J. Stark, M. D., p 263 of this volume.

By returns already received at the office of the Director-General of the Medical Department of the Navy, and with which I have been favoured by Sir William Burnett, it appears that epidemic catarrh or influenza prevailed on foreign stations during the year 1847, as undernoted In January and February, on the coast of Portugal and south coast of Spain. January, February, March—in Newfoundland and New Zealand. February, March—at Valparaiso. April—Coast of Syria. July, August, September—West Coast of Africa south of the Equator. August—Hong-Kong.

Coryza, gravedo, destillatio, and other forms of catarrh are described by the ancients. The disease is called *pose* in old English writers from the Anglo-Saxon *gepose* (heaviness, stupor). Epidemic catarrh is mentioned by Targioni Tozzetti in the Chronica Meteorologica of Tuscany under the years 1323, 1328, 1358, 1387. Cullen cites other instances in his Nosology‡ under the years 1510, 1575, 1580, 1591, 1658, 1675, 1679, 1708–9, 1712, 1729, 1732–3, 1737, 1742–3, 1748, 1758, 1762, 1767, 1775, 1779–80. Ozanam adds the years 1239, 1311, 1327, 1400, 1403,

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* Bulletin de l'Acad. Royale de Med. T. 13, n. 12, 13. Mem. from Dr Renault, surgeon of the Lousquor.

† Letter dated 25th November 1847, in the Gazette Médicale.

‡ Art. 39, Catarrhus a contagio.

1438, 1482, 1505, 1557, 1597, 1627, 1669, 1691, 1695. Some of these epidemics, and others which he describes, were apparently local.*

The disease when severe runs into inflammation of the lungs and pleura, and there can be no doubt that several of the epidemic pleurisies and pleuri-pneumonias of the middle ages were what is now known as influenza. The malignant pleurisy at Venice in 1535, which pervaded Brescia and Lombardy in 1537, as well as the epidemic pleurisy of Switzerland and Upper Italy in 1551, are examples.† The more celebrated epidemic of 1564 is said to have appeared first in England; to have spread thence to the Netherlands, Holland, and Switzerland. Patients died in 3–6 days, after violent delirium, coma, apoplexy. Vast numbers were victims of the epidemic; and physicians, who at the time began to examine bodies after death, learned that "inflammation of the substance of the lungs may be combined with pleurisy." Sydenham notices that peripneumonia and pleurisies committed great slaughter at the end of March 1665—the year of the great plague.‡ The winter had been excessively cold, and with a dry frost lasted till spring; a sudden thaw took place in March. Influenza was epidemic in England in 1657; it broke out in April. (*Willis de Febribus*). Influenza described tolerably well by Sydenham as *Tusses epidemicæ anni* 1675 *cum pleuritide et peripneumonia supervenientibus*,‡ and distinguished by him from pure pleurisy, spared scarcely anybody; attacked all ages and temperaments; and seized whole families together (*integras simul familias pervadentes*). It was dangerous. The previous years, 1674, 1675, are referred to as a period of great dearth; the harvest of 1673 proved defective; and wheat, which was 36s. per quarter on Lady-day 1673, was 64s. on Lady-day 1674. The epidemics of 1709 and 1729–30, in Germany appear to have been well described by Hoffman. Epidemics in 1728, 1733, 1742, 1758, 1762, 1775, 1782, 1788, 1803, 1831, 1833, and 1837 have been mentioned; and several have been fully described by English writers. An abridged account of them is given elsewhere from the original accounts. The symptoms in these epidemics only differed in severity, and from complications with other maladies.

Influenza, like small-pox, probably always exists; in ordinary circumstances it is confounded with inflammation of the air-tubes, yet in London from one to five deaths have been directly referred to it nearly every week since the new London tables were published. Like other zymotic diseases it becomes, at intervals of some years, epidemic; that is, it attacks the people generally of all classes. Its epidemics are distinguished by the numbers they assail; by affecting the same persons more than once; by being most fatal to the aged, and therefore causing the deaths of females to exceed those of males during their prevalence; by great differences in the severity and fatality of their attacks; by the rapidity of their course and passage from place to place. After the mortality they occasion becomes apparent in London, it attains a *maximum* in the second or third week; and the mortality falls to the average in the sixth or seventh week. Influenza appears to be generated in ill-organized camps, in crowded, ill-cleansed cities; and to be most fatal among people who have for some time before been depressed, ill-fed, or ill-supplied with vegetables, as after hard winters and in war.* It rages in cold and hot, moist and dry weather, but most frequently breaks out after a thaw, or with violence after a fog, generally the result of cold streams of air mixed with warm air, and a calm. The saturation of the atmosphere favours the transformation of all organic matters; and those of a zymotic character among the rest. Extreme cold only never raises the weekly mortality in London above 1500; extreme heat still less; intermediate changes affect the mortality but slightly in ordinary circumstances; November fogs occur every year without giving rise to influenza; in November 1847, the weather was nearly the same all over England, yet influenza did not break out simultaneously. When once generated, the epidemic spreads through the air. The great epidemics generally travel from (1) Russia, over (2) Germany, (3) Denmark, Sweden, England, France, (4) Italy, Spain,—in from three to six months, and then reach America. Influenza is often associated with other epidemics. It appears to have preceded or accompanied the plague in the black death of the 14th century;† it preceded the great plague of London (1665); it followed epidemic typhus in London, 1803; preceded it in 1837; occurred in the midst of the typhus epidemic of 1847; preceded and followed the epidemic cholera in 1831-2-3. It carries off asthmatic persons, and those suffering from chronic disease; it affects those labouring under other zymotic diseases; in the healthy it quickens the seeds of other maladies, particularly of the lungs. The fatality and duration of attacks vary with age. In some of the late epidemics two in 100 cases attended by medical men are said to have died.* If this was the rate of mortality in London, for 5000 deaths, there must have been 250,000 cases of sickness of not less than seven days' duration. This would be little more than one in eight of the population, but nearly all were affected more or less; and, without taking slight instances, it is probable that not less than 500,000 persons in 2,100,000 suffered in London from the epidemic of 1847.

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* Histoire Med. des Maladies Epidemiques, tome 1, pp. 93—218.
† Sprengel's Histoire de Med. vol. iii. p. 88. And authors cited.
‡ Syd. Opera. Obs. sect. 2, cap. 1, sect. 4, cap 5.

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* "Morbi præsentes a præteritâ temporum conditione fluunt; accipiunt veró etiam differentiam á conditione præsentis: quare utriusque oportet habere rationem." The first part of this profound Hippocratic aphorism, which applies to revolutions as well as epidemics, is often overlooked.

† See Hecker (Epidemics of Middle Ages). The symptoms were not, as he assumes, those of simple bubo plague. The pulmonary symptoms are always dwelt on by the contemporary writers; and Fracastorius, referring to the black death, specifies them exclusively:—

> "Insolita exarsit febris, quæ pectore anhelo,
> Sanguineum sputum exagitans, (miserabile visu!)
> Quartâ luce frequens fato perdebat acerbo."
>
> Fracastorius, Syph. Lib. i. v. 189.

The English physicians of the 18th century agreed in pronouncing influenza contagious. By this they did not mean that it was propagated by contact, but that it was introduced into cities, institutions, and houses in England by persons actually affected by the disease. This notion is, however, too exclusive; the word "contagion," applied to influenza or cholera, is apt to mislead, and to have practically a bad effect. When people ask if a disease is contagious, they generally mean, "Are we more likely to have influenza or cholera if we touch or go near persons labouring under these diseases?" Now, if the matter of contagion is very diffusible, and is distributed equally through the room, the house, the street, the city in which a patient is lodged, no one living in the house, street, or city is much more likely to be infected if he approach the sufferer, than if he remain in absolute solitude, shut up like the grocer of Wood Street during the plague. The matters which excite influenza and cholera are evidently highly diffusible; in a few days influenza spread all over London; it met you everywhere. Nobody therefore has attempted to show that medical men, nurses, or others in attendance on the sick, suffered more than other people. If such should ever be the case, either in the influenza or the cholera epidemics, it will be in rare circumstances, and should never deter the most timid from discharging their duties to the sick.

The earth, it is well known, is surrounded by an atmosphere of organic matter, as well as of oxygen, nitrogen, carbonic acid, and watery vapour. This matter varies, and is constantly undergoing transformations from organic into inorganic elements; it can neither be seen, weighed, nor measured. The chemists cannot test its qualities. Liebig, with all the appliances of the Giessen laboratory, cannot yet detect any difference between the pure air of the Alps and the air through which the hound can tell a hare, a fox, or a man has passed; or the air which observation shows will produce small-pox, measles, scarlatina, hooping-cough, dysentery, cholera, influenza, typhus, plague. These matters may either be in a state of vapour, that is, elastic or inelastic; or, like water, they may exist in both states. They are most probably in a state of suspension,—hang, like the smoke in cities, over the places in which they are produced, but are spread and driven about like vesicular water in clouds. A stream of aqueous vapour of the same elasticity from the Atlantic, passing over England, is in one place perfectly transparent, in another mist, in another rain; so clouds of infectious matter may fleet over the country, and in one place pass harmless by, in another (as influenza in London) destroy thousands of lives. The emanations from the living, the graves, the slaughterhouses, the heaps of filth rotting, the Thames—into which the sewers empty themselves—raise over London a canopy which is constantly pervaded by zymotic matters; in one season this, in another that, preponderating; and the epidemic influenza may easily be conceived either to have broken out spontaneously, to have been conveyed hither by ships, or to have been carried over by the winds; as the cases of the Indiaman infected in the Chinese seas, our own fleets, and the Lousquor in the Mediterranean, seem to imply that *influenzine* may be carried great distances over the ocean from the place of its origin:—

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* See a return by Dr Bain of the mortality at different ages among 317 persons, and a lucid description of the epidemic of 1837, as it appeared in London; British Annals of Med., vol. i. p. 265; and the able Report on the Influenza of 1837, by Prov. Med. Assoc. Trans., vol. vi. Data might now be procured for determining the exact mortality at different ages of complicated and uncomplicated cases.

> "Proinde ubi se cœlum, quod nobis forte alienum est,
> Commovet, atque aer inimicus serpere cœpit;
> Ut nebula ac nubes paulatim repit, et omne
> Qua graditur, conturbat et immutare coactat;
> Fit quoque ut, in nostrum quum venit denique cœlum,
> Corrumpat, reddatque sui simile, atque alienum."

The zymotic hypothesis, here so well stated, explains the phenomena to a certain extent. Still epidemics afford a vast field for research; for much remains to be investigated.* The mariner, however, steers his ship without knowing whence the winds come, whither they are going, or why they are blowing; so, as the returns adduced above establish beyond a doubt the fact, that influenza was four times as fatal in one part of the population of London as it was in another, and that it is much more fatal in a part commonly insalubrious than in a part usually healthy, the course to be pursued is plain. If the means which improve the public health are applied with intelligence and energy, the general mortality can be reduced; and the ravages, either of any future influenza, or of the coming cholera, can be rendered inconsiderable.

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* The inquiry would be greatly facilitated if such meteorological returns as the astronomer-royal now supplies, and weekly tables of the causes of death, were published on a uniform plan in Paris, Berlin, Stockholm, Petersburg, Moscow, Vienna, Venice, Rome, Naples, Madrid, Lisbon, Cairo, Constantinople, and Calcutta.

The piety of the ancients, and of our ancestors, made them consider all plagues the immediate visitations of God's wrath.* And there can be no doubt, that, though as affecting individuals there is nothing now judicial in plagues, they are the results of great national violations of the laws by which the Almighty is pleased to govern the universe. It has been shown, year after year, that the impure air of London destroys thousands of lives—that it makes epidemics fatal: the causes of the excessive mortality have been pointed out; and it has been proved that they admit, to a considerable extent, of removal. Yet the efforts of Her Majesty's Government to introduce sanitary measures were strenuously opposed up to the close of the last session of Parliament. The population of London was left at the mercy of any epidemic that should break out in any part of the world. During six weeks, influenza interrupted business, afflicted hundreds of thousands by sickness, implanted fatal diseases in the breasts of many, and destroyed 5000 of the inhabitants. Are the men who opposed sanitary measures, and declared so loudly that the "city of London, for health, cleanliness, effective drainage, lighting, and supply of water to its inhabitants, cannot be surpassed," satisfied? or will they fight against the public good till cholera drive away their best customers and decimate their own families? Let us hope that, instead of pursuing such a course, they will assist in supplying London with the necessaries of city life which they have enumerated, and which they now know are wanted.

PREVIOUS EPIDEMICS OF INFLUENZA IN ENGLAND.

1728, an epidemic catarrh prevailed in the beginning of January.† 1727–9 was a period of some dearth. Wheat rose from 4s. to 8s. a bushel. The winter of 1728 was almost as rigorous as that of 1709, when influenza broke out in Frederic's new city of Berlin.

1733.† Influenza invaded the northerly parts of Europe before the southerly; was in Saxony, November 26, and lasted in its vigour fourteen days; was earlier in Holland than in England; in Edinburgh than in London; where it lasted in its vigour from about the end of January for about three weeks; the bill from February 3–10, contained in all 1588 deaths, being higher than any since the plague. This is equivalent to 5304 deaths in the present population (2,100,000); for the population in the bills then was about 650,000. The mortality in 1733 was nearly double that in the epidemic of 1847. The deaths in the three years, 1731–2–3, were 25,262, 25,358, [29,233], according to the London bills. The price of wheat was low in the three years; 33s., 27s., and 28s. a quarter at Eton. The epidemic began in Paris about February 1, and lasted till the beginning of April. It raged in Naples and Southern Italy in March and April. It was four months in passing over these parts of Europe. Huxham, writing at Plymouth, says it seized upon rich and poor; scarce any escaped, old or young, strong or infirm, either in the town or country. It was in Cornwall in January, but attacked Plymouth on February 21. It was a Saturday, on which a very great number were as it were suddenly seized; the next day an infinite number felt it, and by the 29th of March everybody everywhere. Few died at Plymouth, and those were chiefly infants and old asthmatic people. In 1728 and 1733, a most vehement cough seized almost all the horses about a month or two before it attacked mankind.—(Huxham.) Under 9 Geo. 1. (1722–3) the workhouse system came into operation, and many parishes farmed out their poor at low rates. The Persians under Kouli-Khan defeated the Turks in 1730.

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* "Morbos tum ad iram Deorum immortalium relatos esse."—Celsus.

† Arbuthnot on Air, p. 193. Huxham de Aëre, An. 1728, 1733, 1743. The dates given in O.S. are converted into new style.

1743.*—Another epidemic raged through all Europe in 1742 and 1743; the disease obtained the name of *Influenza:—per totam Europam hoc vere sub nomine Influenza*, are the words used by Huxham;* hence the learned Sprengel is mistaken when he says that the disease was first called influenza in 1762.† At the end of 1741 remarkable auroras were visible; in February 1742, influenza became epidemic in Saxony; it was at Milan in November, Venice in December, France in March, 1743. In the spring of 1743, it increased the funerals in London to 1448 in one week. The burials in London in the six years 1739–44, were 25,432, 30,811, 32,169, 27,483, [25,200,] 20,606. The winter of 1739–40 was one of extraordinary severity and duration; coals were 2s. a bushel, water dearer, bread was dear, and riots broke out. Fever appears to have been epidemic; at the beginning of 1742 the assize of wheaten bread was 5½d., at the end 4¾d. the quartern loaf. This was cheap at that time. The plague swept off 50,000 of the inhabitants of Messina in 1743.

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* Arbuthnot on Air, p. 193. Huxham de Aëre, An. 1728, 1733, 1743. The dates given in O.S. are converted into new style.

† It was called *grippe* (which is "whim," in French—and also suggests *gripper*, to grip, to seize) in Paris during this epidemic. It had formerly been called coqueluche, ("a cowl,"—also "the reigning fancy,") and in some of the epidemics had been apparently confounded with hooping-cough. The epidemic has hitherto been the occasion of *bons mots* for our *spiritual* neighbours; it will be well worth the while of the public, even for the sake of change, to look at it seriously now, and to support the able men in France, who think that Paris would be all the merrier for being clean and healthy. In the sixteenth century influenza was called "the new acquaintance" in Edinburgh, at the Court of Queen Mary.

The four years 1739–42 were passed in cruel destructive wars, waged by Nadir Shah, Kouli Khan, and the Turks in Asia; Frederick, Maria Theresa, Louis XV., in Germany; the Swedes, the Russians, and the Turks, in their own dominions; Spain and England, in America. Vernon and Wentworth were ordered to return in September 1742, after the disastrous expedition of Carthagena; Anson was away on his fatal, but not unfortunate voyage round the world.

1758. The summer and autumn were warm and dry, the wind easterly, the epidemic influenza made its appearance in Edinburgh in September, was very general in October, at the end of which it began to decline. It did not appear at St Andrews until October 10; nor at Inverness until the middle of the month. It was most fatal at Dundee, Perth, and Glasgow.*

1762. Sir George Baker commences his relation by saying, that on April 4th three persons were attacked in the same house by the same disease; and that by April 24th it had spread through the whole of London. It attacked all indiscriminately, and was fatal to the aged, and particularly to those who were asthmatic. The burials in the bill for the week ending May 4, were 467, and for the weeks following 626, 750, 659, 516, 504. The burials in each of the five years 1760–4 were 19,830, 21,063, [26,326], 26,143, 23,202. Influenza raged in Warsaw at the end of February (the deaths from 30–40 rose to 150 weekly); at Magdeburgh in April, Hamburgh in the beginning of April, Alsace in June, in the Mediterranean among British sailors in July. It was more fatal in Venice than elsewhere. Paris was not visited. It was unknown in Britain until it invaded London; and in many cities, notably in Norwich, Lincoln, Leicester, Exeter, those first attacked were strangers lately arrived from London. A dysentery followed in July, and is described by the same author.† The war with France ended in the peace of Fontainebleau, November 3.

1767. Dr Heberden mentions an epidemic cold, resembling that of 1762 in London, in June and July, when it entirely ceased. It brought on anginas, pleurisies, and peripneumonias. Both sexes and all ages were attacked. It occurred in other parts of England about the same time. The season preceding this was remarkably cold, while that in 1762 was remarkably warm.‡

1775. Dr Fothergill states that the influenza appeared about the beginning of November. Some in addition to the common symptoms had diarrhœa. The mortality was trifling considering that so many persons were attacked. Horses and dogs were much affected by the complaint. Sir John Pringle had sore throat himself, and no cough. He met with similar cases. He heard of this complaint in France, Italy, and the Low Countries; and inferred with Sir G. Baker that the "*sensible* qualities of the air had most probably no share in producing this complaint." Dr Heberden saw the first patient on October 28th; and at the end of three weeks few were attacked who had escaped it up to that period. Two persons had eruptions like scarlatina. It "did not kill, but hastened the death of those who had other complaints." Sir George Baker saw it first about October 20th. Dr Haygarth of Chester remarks that "it assumes a milder form in warm weather;" Dr White of York that the epidemic "was checked by severe frost and snow."* The burials in London were 20,514 in 1775; which was below the average of that period. This was towards the commencement of the American war.

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* Med. Obs. vol. ii., Comm. by Dr Whytt, Dr Alves, Dr Simpson.

† De Catarrho et de Dysenteria. auct. G. Baker, Lond. 1764.

‡ Med. Trans. vol. i. p. 437.

1782. This epidemic was reported on by a committee of the College of Physicians, and by a great number of medical men all over the country.† Influenza, in September 1780, broke out in an East Indiaman sailing from Malacca to Canton, where it was raging violently at the same time that they had it on board. It broke out in the British army at Negapatam in November 1781, (Coll. com.) It prevailed at Moscow in December 1781, and January 1782; at St Petersburgh in February 1782. It was traced to Tobolski, and supposed to have been brought from China. It reached Denmark at the end of April; many died of it at Copenhagen before May 11th.—(Dr Gray.) It was in Scotland and Ireland, later than in England (June); France in June and July; Italy in July and August; Portugal and Spain in August and September; afterwards in America.—(Dr Grant.) It was most prevalent in London in the 4th week in May. Dr Grant thinks that it first prevailed in London; the committee of the college are uncertain, and say that it did not observe any regular progression from any one point of the compass to another. They note, however, that it appeared at Newcastle-upon-Tyne at the end of April, and raged in May and June; in London, May 12–18; Norwich and Bury, the middle of May; Hadleigh, in May, but was worst in June; Kingston and Guildford, May 20th; Portsmouth in May; Oxford, the third week of May; Chester, May 26; Plymouth, May 30; and Cornwall (west), May 19; York and Liverpool in June; Edinburgh in May; but not at Musselburgh, within five miles of Edinburgh, until June 9; nor at Glasgow until June. It appeared earlier in towns than in villages: in villages earlier than in detached houses. Dr Parr of Exeter states, that the epidemic first appeared in Devon, May 23; was prevalent in June; disappeared in July; lasting in the county about seven weeks. Dr Scott says it was first observed in Northumberland on June 1; and then spread all over that county. It was fatal. In the beginning of June it affected the whole of the regiment in the castle of Edinburgh within a few days. The disease was almost universal in London, four-fifths of the people had it; few died, except the old and asthmatic. It seldom held any one more than a fortnight; some had three or four relapses. It generally went through whole families, seizing all simultaneously; in some instances, however, portions of families escaped. Of 700 boys at Christ's Hospital, only fourteen had it, and those not severely. Three families, consisting of seventeen persons, arrived on the same day at an hotel in the Adelphi all in perfect health; the next day they were all affected with symptoms of the reigning disease. In an hospital containing 170 persons more than 100 were attacked within twenty-four hours; few escaped afterwards.* A sergeant of the 10th Foot Grenadier Guards, visited London on furlough, when the disease raged there. He returned in a few days to St Albans affected, and communicated the disease to the people in whose house he had his billet. It was the first appearance of the disease there, and thence spread rapidly all over the town.† In this case much would of course depend on the care and judgment of the observer. Two Hanoverian regiments, which arrived from Minorca on July 23, at Plymouth, where influenza had ceased for ten or fourteen days, had nothing of the influenza before or after their arrival. On the other hand, a family who arrived in England from the West Indies in September, was attacked by influenza in October. Carmichael Smith observed petechiæ and gangrene in two fatal cases; others terminated in malignant fever. Mr Jacob met with erysipelas of head, abscesses in various parts, parotid and axillary, miliary eruption, pleurisy. In Cambridge it had a putrid type, with delirium. Some died suddenly, some had diarrhœa, (Dr Macqueen). All agree in calling it a universal disease. It is worthy of remark, that in March 1781, a distemper broke out among horned cattle; who were directed to be killed and buried, by order of Council, to prevent infection. The Gazette of August 27, 1782, contains dreadful details of the ravages of plague at Constantinople. Sir G. Baker observed, that influenza was more fatal in France, Holland, and Germany, than in this island. The bills in London were not much swollen. The burials in four weeks ending May 28, were 299, 307, 336, 390; in five weeks ending July 2, 385, 560, 473, 434, 296. Neither the mortality of London nor of England was increased in 1782, above the average of the period; the burials in London, 1780–83, were 20,517, 20,709, 17,918, 19,029; in all England, 191,736, 195,902, 187,152, 188,264. The inefficiency, however, which it caused may be conjectured, from its effects in the fleets employed at the close of the American war.

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* Med. Obs. and Enq., by a Soc. of Physicians in Lond., 1775. Queries were circulated by Dr Fothergill.

† Medical Transactions, vol. iii. p. 54, and Medical Communications, vol. i. p. 2.

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* Med. Trans., vol. iii. p. 59.

† Dr Hamilton, Mem. Med. Soc. of London, 1782.

On the 2d of May 1782, Admiral Kempenfeldt sailed from Spithead with a squadron of ships under his command, of which the Goliah was one, whose crew was attacked with influenza on the 29th of that month; the rest were affected at different times, *and so many of the men were rendered incapable of duty by this prevailing sickness, that the whole squadron was obliged to return into port about the 2d week in June*, not having had any communication with any shore, and having solely cruised between Brest and the Lizard. The gallant Kempenfeldt went down in the Royal George, August 30th of the same year.

About the 6th of May, Lord Howe sailed for the Dutch coast with a large fleet under his command; all were in perfect health towards the end of May. The disorder first appeared in the Rippon, and in two days after in the Princess Amelia; other ships of the same fleet were affected with it at different periods, some indeed not until their return to Portsmouth about the 2d week in June. This fleet also had no communication with the shore until their return to the Downs, on their way back to Portsmouth, towards the 3d and 4th of June.

In 1788 there was an epidemic. It raged in Paris in July, Vienna in November. Dr Willan, in his reports for 1797 and 1800, mentions epidemic catarrhs; which he says were improperly termed influenza by many practitioners—as they were not *infectious*, nor general.

In the beginning of December 1799, influenza became epidemic in Moscow, and soon after that at Petersburgh. In January 1803, it was epidemic in Paris.—(Ozanam.)

1803. Dr Andrew Duncan, Edinburgh, met with cases of influenza on February 10; it was very common in March, contagious, not very fatal. He says it was common in London, February 3; its progress from Paris to London, London to Edinburgh, and from Edinburgh, is a proof that it is contagious. Dr John Scott says that it appeared at the end of March in the Isle of Man, it attacked patients already confined to their bed; it promoted phthisis, induced miscarriage. At Bristol, Dr Carrick says it was provoked by exposure to cold, and sharp east winds, prevalent at that time, March. There had been a scarcity in 1800 and 1801; a famine and typhus followed. The annual deaths in London, 1799–1804, were 18,134, 23,068, 19,374, 19,379, [19,582], 17,038. The annual rate of mortality among females in all England was 2.62, 2.52, 2.51, 2.19, in the four years 1801–4; after the year 1803 a remarkable decline in the general mortality of the country took place; and this was only disturbed by higher rates of mortality than 2.2 per cent. in 1807, 1808, 1810, 1825, 1826, until 1831.

Bateman in his reports, extending from 1804 to 1816, noticed, in the three months ending February 1813, cases resembling the commencement of the epidemic influenza. The winter of 1814 was the severest since that of 1794–5; a dense fog in December 27; 1813 was followed by fog, snow, and frost in January, a partial thaw occurred, then the Thames was frozen over in February, and great multitudes were entertained in booths on the Thames. On many days the temperature was as low as 15°. The frost suddenly disappeared in March 20. About 200 of the dispensary cases assumed the form of acute catarrh; many were entered as catarrhal fever, 50 put on the character of the most active pneumonia.*

1831. An influenza epidemic began in London about the middle of May, and continued during June and a part of July, under very hot though damp weather.† The burials in the London bills of the parish clerks, now become very defective, were 21,645 and 25,337, in the two years 1830, 1831; the mortality in the whole kingdom was 2.09, and 2.25 per cent. in the same years. The Polish insurrection broke out in November 1830; Warsaw capitulated in December 1831. Cholera raged in the armies in 1831; and was preceded by influenza. Cholera broke out as an epidemic at Sunderland, October 1831; the deaths of six persons were ascribed to it in the London Bill for the week ending February 14. Cholera raged with considerable violence in March and April for 9 weeks, subsided in May and June, and broke out a second time at the end of June, raging with considerable violence for 14 weeks, through July, August, and September. No cases were returned in February, March, April, May, 1833.

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* Bateman's Reports, pp. 212, 226, 232.

† Dr Holland's Medical Notes, p. 200.

1833. Influenza broke out as an epidemic in the spring. The word first found its way into the bills of the parish clerks for April 30, 1833. The burials returned for the eight weeks ending April 9, to May 28, were 321, 587, 796, 961, 940, 874, 575, 311. The cause of the excess in the mortality was influenza; to which, however, only 89 deaths were directly ascribed. "It spread over every part of Great Britain and Ireland during the spring of 1833, after having previously appeared in Russia and the northern parts of Germany, inflicting great mortality in every part of its course."* Cholera broke out in London again in July, epidemically, and prevailed through August and September.

1837. Influenza was epidemic during the month of December 1836 in Russia, Sweden, and Denmark. In Copenhagen, 30,000 persons were under the disease at one time (Dr Otto). In Scotland it was observed earlier than in England. It had been prevalent a fortnight in London before it reached Brighton. It appeared also in Lancashire, Cheshire, and Gloucestershire, from 7 to 14 days later than in London. Though very general, some places in the neighbourhood of infected places seemingly escaped. Dr Holland, from whom this account is taken, goes on to say:—

"The epidemic showed itself in Paris about a month later than in London, having previously appeared at Calais and other intervening places. I saw patients who, *on their journey from Paris to London, had come upon an infected town, and been suddenly seized with the malady.* At Paris it was stated to have affected at least half the population, but seemingly with less mortality than in London. (?) The epidemic spread gradually over other parts of France. About the end of February it affected the northern coast of Spain, the more conspicuous there from its influence on the events of the civil war, then raging in Biscay and Navarre. Almost at the same time it appeared at Lisbon—a new occurrence in that city—spreading *successively* to the several towns which lie upon the Tagus, even to the Spanish frontier. Dr Leitao, who has narrated its progress in Portugal, seeks to show that it is contagious, and was brought directly from England to the British squadron in the Tagus, in the vessels of which it first appeared. The same idea prevailed in Biscay as to its manner of importation. The disease reached Madrid about the end of March, and prevailed there the whole of April.

"In Germany the influenza appeared at Berlin in January, affected Dresden somewhat later, and Vienna and Munich a fortnight after Dresden. At Hamburgh, where it appeared in the first days of January, Dr Rothenburgh states, that more than half the population was attacked. I do not possess any information as to its progress in Italy. In Malta it first showed itself about the 1st of June.

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* Medical Notes by H. Holland, M. D., 2d ed., p. 196.

"A remarkable fact is, that an epidemic, having the characters of the influenza of the northern hemisphere, prevailed at Sydney and the Cape of Good Hope in the latter part of 1836; the time thus corresponding with its earliest appearance in the north of Europe, though under a date of *season* wholly different. Sir John Herschel informs me, that the weather was warm and apparently genial, at the time when almost every individual in the Cape district was suffering under the epidemic. The malady spread up the country as far as Gnadenthal, producing there considerable mortality in the Hottentot population."*

Cholera was epidemic in Germany, but not in Eng.and, during 1837. Typhus was epidemic in England, and the mortality of females in 1837,† through the country, was 2.52; or considerably higher than it had ever been since 1801. The mortality in the year 1832, when cholera was epidemic, was only 2.42 per cent.; in 1833, when cholera and influenza were epidemic, it was 2.30 per cent. The new system of registration came into operation in July 1837; and the mortality of females never exceeded 2.10 in any of the eight years 1838–45; except in 1840, when it was 2.20, and in 1838, when it was 2.14 per cent. Influenza was slightly epidemic from February 20, to April 24, 1841; but the weekly deaths by the new tables never exceeded 1079, and ranged from 780 to 1079, during that time. Influenza prevailed to a slight extent at the end of 1846, and the beginning of 1847.

Huxham and Arbuthnot described the great epidemic influenza of 1733; we take the following passage from Arbuthnot, as the account is in a popular style.

XI. "There have been of late two remarkable instances of the influence of the air in producing an epidemical disease, perhaps over the greatest part of the surface of the earth; the first happened in the year 1728, the last in the latter end of the year 1732, and beginning of 1733; which being the more recent and remarkable, I shall give a short description of it, till a more particular one can be procured from the collected memoirs of the several countries which it invaded, of which I have seen only a few.

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* Medical Notes by H. Holland, M. D., pp. 198–9.

† The mortality of females is taken in these comparisons extending from 1801–37. The male population—on account of the military—could not be so accurately determined.

XII. " The previous constitution of the air was in *England*, and in the greatest part of Europe, a great drought, which may be inferred from the failure of the springs, in the abatement of the *fresh water* in all its usual currents and reservoirs, which are the best measure of the quantity of moisture falling from the *clouds*. What is most generally taken notice of in the accounts I have seen from *Germany*, *France*, and some other places, was, that the *air* in the beginning of winter, especially in November, was more than usually filled with thick and frequent fogs, the *matter* of which was not precipitated upon the earth in rain, snow, or any other fruits of the air. *Fogs* are so usual in *this* country in November, that there was nothing particular observed about them that I know. But there was hardly anything fell from the *clouds* during the month of November, except a very small quantity of snow, attended with a frost of no long duration; and this was all the winter we had. In the *northern* parts of France, there was a very small quantity of snow, which lasted from their 15th and our 4th of November till after Christmas. This was succeeded by southerly winds and stinking fogs, during which there was observed by some chirurgeons a great disposition in wounds to mortify.* Both before and during the continuance of the disease in England, the air was warm, beyond the usual temper of the season, with great quantity of sulphureous vapours, producing great storms of wind from the south-west, and sometimes lightning without thunder.

XIII. " As to the times of the invasion of the disease, they were different in different countries. It invaded Saxony and the neighbouring countries in Germany about the 15th of November, and lasted in its vigour till the 29th of the same month. It was earlier in Holland than in England; earlier in Edinburgh than in London. It was in New England before it attacked Britain; in London before it reached some other places westward, as Oxford, Bath, &c.; and as far as I can collect from accounts, it invaded the northerly parts of Europe before the southerly; it lasted in its vigour in London from about the middle of January, $173\frac{2}{3}$, for about three weeks;†—the bill of mortality from Tuesday the 23d to Tuesday the 30th of January, contained in all 1588, being higher than any time since the plague. It began in Paris about the beginning of their February, or the 21st of our January, and lasted till the beginning of their April, or the 21st of our March, and I think its duration was longest in the southerly countries. It raged in Naples and the southerly parts of Italy in our March. The disease, in travelling from place to place, did not observe the direction, but went often contrary to the course of the winds.

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* The late king of Denmark, it is said, had phlebitis; is not there some liability to this disease during an influenza epidemic?

† All the dates are in old style; January 15, 1732, is therefore January 26, 1733, new style, &c. The French adopted the Gregorian reform of the calendar in 1582.

XIV. "The uniformity of the symptoms of the disease in every place was most remarkable. A small rigor or chilliness succeeded with a fever of a duration (in such as recovered) seldom above three days. This fever was attended with headache, sometimes pains in the back, thirst in no great degree, a catarrh or thin defluxion, occasionally sneezing, a coryza, or running at the nose, a cough, with expectoration of a thin petuite at first, and afterwards of a viscous matter, in which, if there was observed a clear oily matter, it proved generally the case to be mortal, for this clear matter was purulent. These were the most common symptoms. But a great many during that season were affected with spitting of blood, pleurisies, and inflammations of the lungs, dangerous, and often mortal. In some places, particularly in France, the fever after six or seven days ended in miliary eruptions; in Holland, often in imposthumations of the throat; in all the blood was sizy, and everywhere the disease was fatal to aged people. What was observable was, that the fever left a debility and dejection of appetite and spirits, much more than in proportion to its strength or duration, and the cough outlasted the fever in some more than six weeks or two months.

XV. "There was, during the whole season, a great run of hysterical, hypochondriacal, and nervous distempers; in short, all the symptoms of relaxation. These symptoms were so high in some as to produce a sort of fatuity or madness, in which, for some hours together, they would be seized with a wandering of their senses, mistaking their common affairs; at same time they had not any great degree of fever to confine them to their beds; but in several who were thus affected, the urine was observed often to change from pale to turbid alternately, so that there was some fever, though I did not observe nor hear that the bark was effectual, but the saline febrifuge draughts had generally a most surprising good effect. Since this disease has been over, the air has continued to be particularly noxious in diseases which affect the lungs, and for that reason occasioning a great and unusual mortality of the measles, at the rate of forty in the week, from which one has reason to expect some specialities in the diseases of the succeeding season.

XVI. "The remedies commonly successful in this epidemical catarrhous fever, were bleeding, sweating, promoted by watery diaphoretics, blisters, and the common pectoral medicines; and what I observed before, febrifuge draughts of salt of wormwood, juice of lemon, &c. I have not particulars enough to enable me to enter into the etiology of this distemper.

XVII. "It was a matter of fact, that there was a previous ill constitution of the air noxious to animal bodies. In autumn, and long afterwards, a madness among dogs; the horses were seized with the catarrh before mankind; and a gentleman averred to me, that some birds, particularly sparrows, left the place where he was during the sickness.

XVIII. "The previous great drought, as has been observed before, must have been particularly hurtful to mankind; great droughts exert their effects after the surface of the earth is again opened by moisture; and the perspiration of the ground, which was long suppressed is suddenly restored. It is probable that the earth then emits several new effluvia hurtful to human bodies; that this appeared to be the case by the thick and stinking fogs which succeeded the rain that had fallen before.

XIX. "It is likewise evident that these effluvia were not of any particular or mineral nature, because they were of a substance that was common to every part of the surface of the earth; and therefore one may conclude that they were watery exhalations, or at least such mixed with other exhalable substances that are common to every spot of ground.

XX. "Lastly, it is agreeable to experience that watery effluvia are hurtful to the glands of the windpipe and the lungs, and productive of catarrhs."*

Art. VII.—*Account of a Case of Hysteria, in which nearly all the Spasmodic Diseases were successively and repeatedly simulated.* By R. U. West, Esq., Surgeon, Hogsthorpe, near Alford, Lincolnshire.

On the 8th of September 1842, I was sent for to visit Miss Susannah A——, the daughter of a respectable farmer residing in this parish. She was a slightly-formed delicate looking girl, aged nearly 12 years; rather tall for her age, with fair hair and complexion. She had for some months before complained of occasional pain in the side, with now and then slight difficulty of breathing. On this occasion the difficulty of breathing was a good deal increased, every inspiration being accompanied with a wheezing noise. I looked upon the affection as spasmodic, and prescribed accordingly.

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* Arbuthnot upon the Air, and its effects upon the human body, chap. 7, page 193. Tonson, Strand. London, 1733 and 1751.

Sept. 9th. There was more pain about the chest; breathing as before. I applied a blister.

Sept. 10th. She felt better; but observing a kind of smile on my patient's countenance, it struck me that the case was one of hysteria, which was proved to demonstration by my taking advantage of the involuntary imitation which is characteristic of that affection; for when I asked if she felt inclined to cry, she immediately began to weep and sob most bitterly; and again, when I suddenly said, "Now, I think you had better laugh than go on in that way," she began to laugh quite heartily. Perseverance with antispasmodics was still indicated.

The next evening, September 11th, being Sunday, I was sent for suddenly. I found my patient in a violent hysterical paroxysm, which, notwithstanding the application of mustard poultices to the nape of the neck and the calves of the legs, continued unabated for more than four hours. This paroxysm was characterized by the ordinary series of screamings, laughings, cryings, chokings, and strugglings with her attendants, being accompanied by the usual inability to swallow. I directed Mrs A—— to apply mustard poultices again in the event of a recurrence of the fit, and left my patient. The next evening the fit came on at the same hour, but the mustard being applied at once, it lasted only about an hour. This kind of counter-irritation was applied every evening for the remainder of the week, but by the Sunday following, September the 18th, its good effect was almost entirely lost, as the fits had become every evening longer and longer. During this week I had been ringing the changes on the different antispasmodics without the slightest benefit, besides blistering the nape of the neck.

Throughout the next week the fits continued to recur every afternoon earlier and earlier; they were also every day longer and more violent, being attended with screamings loud enough to be heard at the distance of a mile; and the patient could not be hindered from frequently running out of doors, and exhibiting various strange antics in her father's yard, such as running nimbly along the tops of some palings, and performing similar feats of extraordinary and unnatural agility.

This brings matters down to about the 25th of September. There now began to be a short fit every morning, which at first was only about ten minutes in duration, but gradually increased in length until the 7th of October, when, after an emetic, which was given to her on that day, the morning fit lasted until the time for the afternoon one to begin; and henceforth she had but one series of fits in the twenty-four hours, lasting, at this date, from six in the morning until five in the evening.

On Sunday the 2d of October, a very marked change took place. In addition to the continued hysterical condition, she began to have, regularly every hour, a paroxysm lasting about ten minutes, in which she lay extended on the sofa, perfectly rigid and perfectly straight. The extensor class of muscles chiefly was excited; the arms were held straight upwards, and there was a continual snapping of the jaws. Dr Mason Good describes a somewhat similar condition under the name of *ecstasis*. It was not *catalepsy*, for the limbs were perfectly immoveable. This state ceased quite suddenly every time; the patient would jump up from the sofa, and, relapsing into the simple hysteric state, would continue running about, laughing, crying, and screaming until the next rigid fit.

The next Sunday, October the 9th, there was another change. A fit of *ecstasis* came on every hour, in which the position was the same as in the last described; but it was attended with tremblings, and there was no snapping of the jaws; and in place of the latter symptom, she would, every minute or so, raise herself up and knock her head backwards on the sofa three or four times with great violence. We called these *knocking-fits*, for want of a better term. Her relapse into the simple hysteric state from every one of these fits was preceded, first, by a peculiar *curling* motion of the hands, difficult to describe,—she looked as if she were *unscrewing* herself; by a sudden turning on the face; and lastly, by a *gasping* motion of the jaws.

On the following Sunday, October 16th, another change took place. And, in fact, it began to be remarked, that a change of some kind took place every Sunday; and by this time it was looked for. She had a spasmodic fit every hour, as during the two preceding weeks, but it was now complete *emprosthotonos*, attended with a moaning noise, a ghastly grin on the face, and a chewing motion of the jaws, during which it was necessary to place something between the teeth lest she should bite her tongue; this grin being every now and then exchanged for another form of spasm, in which the mouth was fixed rigidly wide open, and the eyes were staring and unwinking. This complication I called *inverted trismus*, for want of a more appropriate appellation. While in this condition of emprosthotonos, she would make painful attempts to rock herself from side to side. After a day or two had been passed with these fits, we thought of putting her into a child's cradle as soon as the spasmodic state came on, by which means the rocking propensity was easily satisfied, and her apparent distress was much diminished. She came out of every one of these fits by the same process as out of those of the week previous.

The following Sunday, October the 23d, in addition to frequent fits of emprosthotonos, she began to have some *cataleptic* seizures, each of which was prefaced by a strange series of manœuvres. Thus,—she commenced, after a trembling of the hands and head, by leaping wildly on the sofa, and then rather *flying* than running from one side of the room to the other, returning to the sofa backwards for another fly. Two of her sisters were usually obliged to attend, one on each side of her, during these *flying fits* as we called them, and hard work it was for the poor girls, for they were soon almost out of breath. These flyings, after lasting about five or ten minutes, were always terminated by a sudden dropping of the head on one shoulder. She was then carried off by her attendants to the sofa, in a rigid, cataleptic state. Her limbs could be bent, but it was like bending a piece of lead, and in whatever position they were placed, so they remained. While lying in this condition, she crowed like a cock during every inspiration. This crowing gradually ceasing, a trembling of the arms came on, which increased in intensity until the movements had the appearance of *epilepsy*. After a few more minutes these movements ceased, and then after a rapid winking of the eyes she fell asleep, in imitation of the coma which usually follows epilepsy. After about five minutes of this sleep, she rose up, stretched herself naturally enough, and then relapsed into the hysteric state again.

I may here describe the manner in which the whole series of fits invariably terminated every day at this stage of the affection. From being hysterical, she would become emprosthotonic for about one hour, being rocked with the grin on the face. Towards the termination, she would have two fits of *inverted trismus*. Then the *curling* and other movements, described above, would take place; after which she would jump up quickly, snatch a pillow from the sofa, and walk into the kitchen. She would then march seven or eight times round the table, singing all the time, and finishing her march by suddenly jumping like a cat into the cradle. She would then leap up, march round the table six or seven times as before, and jump into the cradle again; for the third time she would repeat her marching and jumping into the cradle; she would then stretch herself out, and commence wringing her hands for a minute or two, after which she would, softly, and in a whisper scarcely audible, mutter, "I am better."

The duration of the series of fits had, up to this time, continued to increase steadily, and now lasted about nineteen hours, from early in the morning till late in the evening. One day in the middle of the week, beginning October the 23d, after being *right*, as we called her natural state, for about an hour, the fits returned again the same evening, and she now had them in the night instead of the day. The week after, she would have occasional short hysteric touches, lasting a few minutes, during her lucid state. These gradually got worse, till, at the beginning of another week, when I called (on the 7th of November), I found that she had only three quarters of an hour free from her affliction; and, be it remembered, it was only during this brief lucid interval that she was able to take any nourishment.

Monday, Nov. 7. I this day saw the poor girl take her hasty meal—a small fried sausage, a piece of bread, and a single cup of coffee; after which I witnessed the following series of phenomena.

She jumped up hysterical; she laughed cheerfully, then moaned plaintively, put on a vacant look, suddenly turned up her eyes, and fell back on the sofa. There was a violent heaving of the chest and abdomen. She then rose up into the sitting position, and commenced what was apparently an *epileptic* fit. It differed from true epilepsy in this, that the twitchings of the eyes were not synchronous with the movements of the arms, and these were more like impatient jerkings than genuine epileptic twitches. Besides, there was an occasional voluntary rectification of position, during which one or two jerks were missed. In short, the patient had the air of one who was making semi-voluntary movements for the purpose of relieving some uncontrollable sensation. The mouth was distorted sideways, and the jaw continually opened and shut gently. But we called these fits epileptic, because we wanted a more appropriate name for them. From this state, she stretched herself out on her back, while a frothy saliva issued from between the closed teeth. She then suddenly jumped up with a wild cry, and the room was hastily cleared for a flying fit. The cataleptic state next followed in the manner above described, and she then became emprosthotonic. Out of this state she *unscrewed* herself, and became hysterical. I then came away. Her friends told me on this occasion, that the curling motion was sometimes ineffectual, for, after the apparent gaspings which followed the unscrewing movements had continued some time, it now and then happened that, instead of the hysteric state, she relapsed into the emprosthotonic again. The emprosthotonos to-day seemed mixed with pleurosthotonos. In the night, during this doubled up state, she has her sleep. The grin leaves her face, and she sleeps for six hours at a time rolled up like a ball.

To describe the distress of the family at this period would be quite impossible. In addition to the anxiety naturally caused by the anticipation, that, if the disorder continued to advance, so that the series of one day should run into that of the next, it must prove speedily fatal from the total inability of the patient to take any nourishment at all,—there was the incessant bustle and confusion, all night and all day, caused by the actions of the unfortunate girl. When not affected with any of the spasmodic fits, there was an under-current, so to speak, of hysteria constantly going on with her. This permanent hysterical condition, which was the foundation of the whole disorder, had some peculiarities which are worthy of record. The appearance and actions of the poor girl were quite those of a maniac. She was wont to converse in the most animated way with those around her, but always preferred the most abhorrent subjects. She had nicknames, abuse, and even blows for every one that approached her, if by either word or deed her capriciousness was at all excited; for example,—if any one offered to shake hands with her on leaving the room, she would double her fist, strike him a heavy blow on the face or shoulder, and with an exclamation, "You old devil!" run out of the room. All this was so far from being her natural disposition, that she appeared throughout this strange affection to have two distinct and separate existences. In the one, she was a mild, weak, timid, gentle girl, not able to walk or stand by herself; in the other, she was a turbulent, noisy, strong, voluble hoyden, keeping every one on the alert with attending, now to her caprices while merely hysterical, and now to her entirely involuntary actions when affected with the spasmodic movements. These existences were distinct in another sense, for whatever she said or did while in the one state, she had no recollection of while in the other; this recollection invariably recurring with the return of that state in which the particular event took place. This peculiarity was frequently tested, and always with the same decisive result. But the point of dissimilarity between the two states which was of most importance, and which threatened the most serious consequences, as the complaint reached its acme, was the fact that it was only during the natural or non-hysterical state that the patient could take any food; that inability to swallow, which, in a greater or less degree, always attends the hysterical paroxysm, being well marked and complete throughout the whole duration of every series of fits. The poor girl's fantastic sayings and doings, while in the hysterical state, reminded one forcibly of the tales of possession of which we read in the Scriptures; another and a malignant spirit appearing to be speaking with the tongue, and acting with the members, of a being who was, in her healthy state, altogether incapable, either morally or physically, of such sayings and doings.

Nothing in the way of treatment had been of the slightest avail in checking the onward progress of the malady. The usual diffusible stimulants, including the fetid gums, were administered, in addition to counter-irritation with mustard poultices and blisters in the first instance, and subsequently with tartar-emetic ointment along the spine. The cold affusion was frequently used. Carbonate of iron in large doses was given, and oxide of zinc, nitrate of silver, and musk, were successively exhibited as tonics; quinine, as an antiperiodic; and purgatives, and alteratives, and full doses of morphine, were all administered alternately and successively. Thinking it possible that tape-worm was the cause of the irritation, we tried large doses of turpentine. But every thing was either perfectly useless, or, like the emetic, which was given one day with the idea, that the shock of it might prevent the accession of the fit, did more harm than good. Suggestions from old women of both sexes among the neighbours were plentiful enough; and it really required a more than ordinary stock of good sense and firmness to resist, as Mr A. did, the entreaties of many, who, maintaining that witchcraft was at the bottom of the business, insisted that all sorts of magic should be tried.

It is needless to say that at length we were forced to give up all medical treatment, for how could we expect a patient to swallow medicine who had hardly time to take a single meal once a day?

To proceed with my narrative:—

Nov. 16*th.* I called at 4 P. M. She had an epileptic fit; went into it from the hysterical state; it lasted perhaps five minutes; she suddenly dropped asleep; awoke in three or four minutes—hysterical again. At 4 h. 20 m., she had another epileptic fit; this went off gradually, being followed by a short sleep. When she awoke, she rubbed and scratched her face, and said it would burst. After resting a few minutes, she commenced wringing her hands violently, and shaking her head, which was premonitory of a flying fit. This took place in a more violent degree than I had ever seen. She then went off into the cataleptic state, which, as I have said, always followed the flying fit; she made the usual crowing noise, but it was sometimes interrupted by an involuntary holding of the breath for a minute at a time. The cataleptic state ceased without running into epilepsy. I was told, that the catalepsy became epileptic only once in the day, namely, after the first flying fit in the morning; that this had been the regular course since I last saw her; that what I had seen, invariably took place at 4 P. M.; that during the emprosthotonic fits of the last day or two, the fits of inverted trismus had been much more frequent; that if she slept very long in one of these states, she had to work hard for it afterwards to make up for lost time, fit succeeding fit with great rapidity; that the process of walking with the pillow continued to take place daily, before she came to herself, and that this day she had had only a quarter of an hour of intermission.

On weighing her to-day, it was found that she had lost only four pounds, which was somewhat extraordinary, considering the small amount of nourishment she was enabled to take, and the violent exercise she underwent every day. The manner in which she was in the habit of coming out of her catalepsy, when it did not run into epilepsy, was curious. The stiffness did not entirely cease until she had herself, with each hand, opened one by one all the fingers of the other hand from a semi-flexed position. The first set of fingers was opened slowly, because the hand which was the agent was still cataleptic, and proceeded by pushing itself clumsily against each finger; the other set was opened very quickly, because the hand which was then the agent was freed from catalepsy, and proceeded very dexterously. When every finger was opened, she would frequently exclaim delightedly, "They're all done now!" as she jumped up with a wild hysterical laugh.

Taking a hint from the success which had attended the use of the cradle in relieving the rocking propensity which accompanied the attacks of emprosthotonos, I recommended that a *swing* should be tried, for the relief of all parties during the flying fits, which were daily becoming longer in duration and more vehement.

Nov. 22d. After 3 P. M. she has now every day *three* independent epileptic seizures, the first a very bad one; each going off into a disturbed sleep, which is followed by a fit of choking and retching, very distressing to all appearance. The hysterical states, with which these attacks alternate, are characterized by much singing in a low tone, and some screaming and laughing. Some time after the third epileptic fit, she goes off from the hysterical condition into a flying fit. They have got a swing for the flying fits, which answers perfectly. They find that if they delay long in putting her into the swinging or flying motion, her agitation increases rapidly; she appears to lose her sight, not being able to see the ropes of the swing, which she can readily do if carried or led to it soon enough; she also holds her breath in a painful way. This condition quickly goes off when she begins to swing; she becomes quite cheerful, laughs, and seems delighted. After swinging at least half an hour, she suddenly sinks into the cataleptic state, the crowing being very loud, and the mouth drawn alternately from side to side, while making an inspiration. She opens her fingers one by one as described above, and then, after being hysterical again for about half an hour, she suddenly doubles up into the emprosthotonic state. She was only ten minutes to-day completely free from her fits. She comes to herself every day at half-past eleven, after walking with the pillow as described above, and begins again with a swinging fit, fit succeeding fit all day and all night.

Nov. 28th. Nearly the same as last report. About ten minutes to spare for eating and drinking, but rather inclined to be hysterical at the same time. She has the same daily series of fits. They have counted thirty-one changes in the twenty-four hours, reckoning each hysterical state as a change, but considering the swinging, the cataleptic, and the accompanying crowing fits as one fit, inasmuch as they invariably form one process. The chokings, which now always succeed every epileptic sleep, are violent beyond description, and last a long time. During the choking there is sometimes a cessation for a few seconds, but then the epileptic twitchings take its place, and altogether the epileptic state is so much worse, that to-day, the three fits of the afternoon, beginning at three and ending at half-past five, have run into one, that is, another fit immediately following the chokings. She has been observed to sweat very much during the night.

Friday, *Dec.* 2*d.* A change manifestly for the better. She has had an hour and five minutes free, and, as soon as she came to herself, declared herself better, and said she was sure she should have more time to-day. She missed one of the epileptic fits in the afternoon. Otherwise the same.

Saturday, *Dec.* 3*d.* One hour and ten minutes free. Rather more rational in the hysterical condition, during which she knitted a pair of garters for "the old fellow," as she calls her father.

Sunday, *Dec.* 4*th.* Two hours and a-half free. Missed the worst epileptic fit, so that she has now only one instead of three, and the chokings have entirely ceased.

Monday, *Dec.* 5*th.* Five minutes more. In the swinging fits she is more rational. She says, "make haste to put me in." She can leave the swing and run to the door. The fits of inverted trismus have ceased during the emprosthotonic attacks. She no longer walks with the pillow.

Saturday, *Dec.* 10*th.* Three hours and three-quarters free, having gained a little every day since last report. Came to herself for twenty minutes in the afternoon.

Sunday, *Dec.* 11*th.* Six hours and twenty minutes in her natural state, all in one period.

Tuesday, *Dec.* 13*th.* Still six hours and twenty minutes. More rational in the hysterical state, and still more quiet in the swinging fits. When I called at half-past six in the evening, she had just come out of an epileptic fit. She went into the swing at a quarter past eight; swung half an hour and five minutes. The cataleptic state which succeeded was attended with occasional twitchings, though at the same time the rigidity was still truly cataleptic; the twitchings came on when she was not able to crow clear. She had also several stoppages, or rather holdings, of the breath, during which the face became flushed.* She opened her fingers before relapsing into the hysterical condition as usual. This day, while hysterical, she sucked an orange and swallowed the juice. She is so weak when quite right, that she can neither stand nor walk by herself, and looks now somewhat shrunken in the face.

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* These holdings of the breath did not by any means seem to depend on a closure of the larynx, but rather on an apparently obstinate refusal to allow the muscles concerned in expiration to act; the breath escaping at last with quite a burst, when it could be held no longer.

She slept for the first time on the sofa last night for three hours after an epileptic fit, that is, not doubled up. This sleep was altogether of a different nature to the short snatches of a few minutes which previously followed each epileptic seizure.

Sunday, Dec. 18*th.* All the past week continued to have from six to six and a-half hours free. This day she had eight hours, but it was interrupted by an interval of a quarter of an hour, during which she was hysterical slightly.

Wednesday, Dec. 21*st.* Still eight hours free. Missed one emprosthotonic fit last Sunday, and another last night, so that she has now only two seizures of this kind in the twenty-four hours. She cast up a sum correctly this morning while in the hysterical state. This afternoon she was knitting after the two short swinging fits which commenced the series. Before she sat down to knit she scratched and fought her sisters. She knitted with great rapidity. When right, she is unable to knit or walk, or make the least exertion, apparently from complete exhaustion. Begins to have some recollection, when in her natural state, of events which have occurred to her when hysterical.

After this she continued to gain ground rapidly. There was generally a decided change for the better on a Sunday. Every week or so missed one or two convulsive or spasmodic fits.

One day in January (1843), when she was before that having her fits all night, viz., from 9 P. M. to 6 A. M., the series, to the surprise of all, commenced at 2 P. M. and ended at 9. This night she went to bed with the family for the first time.

In the beginning of February, the duration of the series was a little more than three hours. She would have three swinging fits, two very long ones, each more than half an hour long, the third lasting not more than five minutes. Each cataleptic fit would be followed by an epileptic attack, though with a short interval between. She would open her fingers, as described before, jump up quite briskly, talk of the time the fit had lasted and cheerfully prepare the pillows herself for the epileptic fit which she knew would follow. At this time, she generally knew beforehand how long each epileptic fit would last. She had now but one slight fit of emprosthotonos daily, namely, just before coming to herself, and it did not last above a minute.

Saturday, February 18. She was but one hour and a few minutes in her fits. She has one very short swinging fit, and finishes the cataleptic or crowing fit before they can get her carried to the sofa. She has no need to perform the process of opening her fingers. She has two short epileptic fits, and finishes off, as before, with a brief attack of emprosthotonos.

Sunday, Feb. 19. Only one epileptic fit, and nothing else.

Monday, Feb. 20. No convulsive or spasmodic fits at all. Hysterical for half an hour.

During this week, Miss A—— daily continued to regain the use of her legs while in her natural state, and she was speedily as well and as strong as ever, with the exception of a slight hysterical attack every evening from half-past eight to nine. Some time in May, her father took her to visit an uncle, who resided at some distance. Returning in the evening, they did not arrive at home till after nine o'clock. She had missed her fit. Upon this hint, therefore, we acted. Every evening she took a ride out on a pony, taking especial care not to come in before nine. Once or twice, when she happened, from not knowing the hour, to come sooner, the fit returned. This system was kept up till the days got too short; and then the little hysterical attack returned as before, every evening.

At the end of the first week in the following September, it being exactly a year from the commencement of the disorder, Miss A—— complained of feeling poorly. A few days after, her evening hysteria began earlier, lasted longer, and was accompanied by an epileptic fit. It was feared that her troubles were about to return, but it was a false alarm. The next evening she had no fit at all.

In September 1844, it being just two years from her first seizure, she had a fit of hysterics, not attended with convulsive twitchings.

Since then she has had no return of her peculiar affection, at least in anything like a periodic form. A piece of ill news suddenly conveyed to her would bring on an attack; but that kind of thing is a different affair altogether. She did not menstruate until she was nearly sixteen; and, since the establishment of that function, she has been entirely free from hysteria in any form.

What was the irritation in this case? If it was uterine, it was strange that it should come on so early, before the average age at any rate; and stranger that the affection should be so much ameliorated, although the patient, having arrived at a proper age, had still not menstruated. And yet her complete restoration at the present date would almost seem to warrant the opinion.

Were the perspirations, which were observed just before the first favourable change took place, critical?

The recovery of this patient without the aid of medicine or remedial means of any kind, is deserving of notice chiefly for this reason. It is obvious that, but for the extraordinary severity of the affection, which, in its acme, did not give us an opportunity to try any plans at all, we should have continued to persevere in some scheme or other, and that that plan, whatever it might have been, which we might have been trying at the time when the malady began to decline, would have had all the credit of the cure. Let this be a lesson for us all.

I omitted to mention, and it was scarcely necessary to do so, that the different excretory functions were performed with sufficient regularity throughout, and in either state indifferently.

With reference to the regularity in point of time of the different seizures, and the steady advance and decline of the malady, it is somewhat remarkable that every *seventh* day should have been so noticeable throughout the whole period. Surely there is more than is " dreamt of in our philosophy" in the connection between the human constitution and the days appointed by Infinite Wisdom for cessation from toil.

Since writing the above, I have had the opportunity, afforded me by the kindness of their distinguished author, of reading two excellent " Essays on the Theory of Convulsive Diseases," by Dr Marshall Hall. It would ill become me to put forth any remarks, which would have the appearance of contradicting any one of the deductions of so acute an observer; and it is therefore with no little hesitation that I venture to observe that I am a good deal staggered by one of the statements contained in these essays.

At page 43 we read :—" As a part of the epileptic seizure of the most pathognomic character, I must briefly notice another symptom, * * * it is the *odaxismus*, or bitten tongue, lip, or cheek." And again, at page 57—" But when laryngismus, and especially when odaxismus occurs, there can be no doubt that the case is epileptic. This last is *the* most pathognomic symptom of this dire malady."

Now, in the case above feebly described, not once only, but some hundreds of times, we had this very symptom, which Dr Hall proposes to term *odaxismus*, occurring as an accompaniment of certain seizures of a tetan*oid* character. I allude to the very frequent attacks of emprosthotonos. There was no mistake about this symptom ; it was one of those which gave the greatest trouble to the attendants in watching the patient. In the first seizure of the kind the tongue was cruelly bitten; it was seen to be necessary to do something to prevent this, and therefore, in every subsequent fit, the handle of a tooth-brush was kept in readiness to be thrust between the teeth. When the fits of inverted trismus took the place of this *odaxismus*, the tooth-brush dropt out of the mouth ; the same occasionally when sleep came on in the emprosthotonic state ; it was, consequently, necessary to watch carefully for the return of the moaning, that the tooth-brush might be replaced at the proper time. Miss A—— keeps this brush as a curiosity; the handle is indeed curiously worn, and indented by frequent bitings.

If these seizures were *tetanic*, then odaxismus may occur in tetanus as well as in epilepsy.

But they were not *tetanic*, they were tetan*oid*, I take it ; that is to say, they were *hysteria*, simulating one of the forms of te tanus. *Odaxismus*, therefore, may occur in *hysteria.*

And yet, if the last paragraph quoted from Dr Hall means any thing, it means that *odaxismus* cannot occur in any other affection than genuine epilepsy.

It will not do to say that the emprosthotonic seizures were simulated epilepsy, unless it be contended that odaxismus by itself, without any other convulsive movement, will constitute the affection. Odaxismus is pointedly said to be only a *symptom.* The rocking propensity in these attacks was not by any means a convulsive movement; it was an effort to relieve some uncontrollable sensation, which was effected equally well, nay better, when the body was moved from side to side by another person, as was done in the cradle. With the exception, therefore, of the movements of the jaw, there was nothing like convulsion present; the whole body was perfectly stiff, the flexor muscles being all contracted.

And again, it must not be said that no conclusion can fairly be drawn from symptoms arising in a simulated affection. The fact of odaxismus occurring at all as a simulated symptom, or in a simulated malady, must deprive it of its claim to be a test of the genuineness of any complaint whatever.

Nor would it be right to say that its occurrence in a single case only ought not to be taken to prove anything. The evidence afforded by one affirmative case must outweigh that of a thousand negative ones.

I repeat that it is with much diffidence that I venture upon these remarks. I make them more by way of inquiry than assertion, as it is possible the matter may be capable of explanation.

Dr Hall insists much on the fact, that complete laryngismus is necessary to cause epileptic twitchings. Now the crowings which occurred in my case were specimens of the imperfect kind of laryngismus, which, he says, may occur in hysteria; and it is a curious confirmation of his theory, that a kind of epileptic twitching took place in the case, when the patient "was not able to crow clear,"—*i. e.* when the laryngismus was more complete. It is curious, as showing that even in hysteria, to which affection the theory is not applied, *simulated* laryngismus may cause *simulated* convulsions.

It may perhaps be proper to state, before concluding, that my interesting patient was seen by several of my medical friends, among others by Joseph Hunt, Esq. of Addlethorpe, who has long since ceased to practise, and is now a highly-respected county magistrate.

This case would have been a capital one for the mesmerizers to have got hold of.

Hogsthorpe, Feb. 22, 1848.

PART II.

CRITICAL ANALYSIS.

Art. I.—*The Works of* William Hewson, F. R. S., *with an Introduction and Notes. By* George Gulliver, F. R. S., Surgeon to the Royal Regiment of Horse Guards. London, 1846. 8vo. Pp. 360. Printed for the Sydenham Society.

In the summer of 1741, a young Scot, at the age of 23, from the parish of Kilbride, in the Middleward of Clydesdale, went to London with the intention of finishing, in the schools of the metropolis, his education as a medical and midwifery practitioner. At that time, Scotland was poor, with little commerce, very imperfect agriculture, and no manufactures deserving the name; while society was in an unsettled and precarious state, partly in consequence of the effects of the Union not yet having been fully developed, partly in consequence of the great body of the older aristocracy being rather attached to the Stewart family, and from ignorance and prejudice little disposed to look with confidence or cordiality to the Hanoverian succession. At this time, the population of Lanark, the county town, did not exceed 2000,* while that of Glasgow, the trading capital of the county, and the largest in Scotland, was only a little above 17,000.† Four years previously, the violence of an Edinburgh mob had proved most unequivocally the weakness and inefficiency of the municipal authorities, in setting law at defiance and taking the administration of the laws into their own hands; and four years after this period, the rebellion in favour of the Stewart family had shown how unable the existing government was to protect both of these towns, and save them from the severities and exactions of an irregular, but enthusiastic and ferocious military force.

In such a state of society, property was not only insecure but of little value. Neither commercial nor agricultural enterprise could be said to exist; and it was not till several years after 1745 that adventure and industry of different kinds met with any encourage-

* 2294, according to Dr Webster's census in 1755.

† 17,034 in 1740.

ment. Among other inconveniences, no means almost of very efficient medical instruction existed in Scotland; and there were not many who could avail themselves of the means which did exist. The medical school of Edinburgh was only in its infancy; and the Royal Infirmary of that city, which has formed so essential a part of that school, was not yet completed.

The following picture of anatomical teaching, as it was carried on in Edinburgh in 1740, is given by William Hunter more than twenty years afterwards.

"In the course of my own studies, I attended as diligently as the generality of students do, one of the most respectable courses in Europe. There I learned a good deal by my ears, but almost nothing by my eyes; and therefore hardly anything to the purpose. The defect was, that the professor was obliged to demonstrate all the parts of the body, except the bones, nerves, and vessels, upon one dead body. There was a fœtus for the nerves and blood-vessels; and the operations of surgery were explained, to very little purpose indeed, upon a dog."*

This delineation is referred to by Mr Drewry Ottley, in the life of John Hunter, as applicable to London. This, however, is not the fact. It is the account of teaching anatomy by the first Monro, which, imperfect as it was, was the means of making many good physicians.

London was in a very different position. Provided with several well-endowed hospitals, some of which had existed for two centuries and a half, the capital naturally and conveniently furnished the means of medical education on a much more extended scale, and in a more efficient form. Yet, even in that large and constantly-increasing community, the means which were provided were not yet turned to the most profitable account. Cheselden had previously read lectures on anatomy at St Thomas's Hospital from 1710 to 1730, after which he appears to have left this duty to be performed by Samuel Sharpe. At St George's Hospital, Dr Frank Nicholls, Fellow of the College of Physicians, who had devoted considerable attention to anatomy and physiology, was teaching that subject, and occasionally surgery, by lectures and demonstrations, it is said, with considerable reputation. Lectures of this description were then the sole means afforded to the student of acquiring information in the elements of medicine and surgery. The lecturer on anatomy gave short, cursory, and not very perfect demonstrations of the most important parts of the human body. He subjoined a few physiological observations and inferences of the most obvious and superficial character; and a few remarks on the most important surgical operations, as trepanning the skull, lithotomy, cataract, and amputation, delivered as opportunity suggested, completed the course, and prepared the nascent physician and surgeon for undertaking the responsible duties of his profession. If we judge from the published writings of the day, as the systems of James Keill, Dr James Drake, Cheselden, and others less known, we shall not form a very exalted estimate of the character of the instruction then given. No distinction was drawn between anatomy and physiology, which were mingled together in a manner something of the oddest; while anatomy itself was taught almost solely in subservience to surgery, or rather to mere surgical operations, especially the great operations of trepanning the skull, cutting into the bladder, and lopping off the extremities in the most expeditious manner. Surgical pathology was not yet in existence, and, indeed, was the offspring of the subsequent century, at the distance of nearly three generations of living men. The knowledge of the nature and causes of aneurism was meagre and imperfect, no one having written anything intelligible on the subject except Freind, several of whose doctrines were controverted by the lecturer at St George's Hospital, the same Frank Nicholls, whom we have mentioned. Of knowledge of hernia or its anatomical causes, there was none. Everything, in short, relating to the means of studying anatomy, physiology, and pathology, whether medical or surgical, was in the most meagre and rudimentary condition. Yet was this the system according to which, with a few months' desultory attendance at one of the hospitals, at that time physicians and surgeons were qualified to enter on the duties of their respective professions.

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* Two Introductory Lectures delivered by Dr William Hunter to his last course of Anatomical Lectures, at his Theatre in Windmill Street, &c. London, 1784. 4to, p. 88.

William Hunter, for he was the young adventurer from Lanarkshire, on his arrival in London, took up his abode with William Smellie, a native of Lesmahago, in the upper ward of the same county, who had not long before come up from the town of Lanark to London, and who was at that time exercising the trade of an apothecary and man-midwife at Pall Mall. Smellie was not yet the great and noted man he afterwards became.* But as a

* In Dr Thomson's Life of Cullen, it is stated that Mr William Smellie carried on in the town of Lanark the mixed occupations of cloth-merchant and practitioner in midwifery. This is extremely probable. For not only was the division of labour in Scottish towns at that time imperfect, but it was scarcely possible for the individual to subsist on one occupation, in consequence of the poverty of the country, and the low rates at which the services of medical practitioners were remunerated, or often not remunerated at all. Smellie is said to have been awkward and unpleasing in manner; and hence not to have risen to the elevation which he might otherwise have attained. He had been nineteen years in Lanark previous to his appearance in London. Smellie was nevertheless an intelligent man, eager for information, and read much; and, as Dr Thomson remarks, "it is agreeable to perceive the laudable pains he was taking, in the obscure situation in which he was placed, to enlarge his general knowledge of medicine, and qualify himself to fill that station in society to which the conscious possession of talent prompted him to aspire."

countryman, from the same county, and a friend of Cullen, his reception of Hunter was most serviceable to the latter. Still more momentous was another circumstance. Hunter possessed from Foulis, the printer, at Glasgow, a letter to Dr James Douglas, known at that time as an excellent accoucheur, a zealous anatomist, and the friend of Cheselden; and this introduction, which enabled Dr Douglas to recognize in the young Scot the elements of genius and that sort of energetic mind which forms a good anatomist, procured Hunter at once a protector and useful friend. Dr Douglas having satisfied himself, on the second interview, of the capacity of William Hunter, invited him into his family, in order to assist in his dissections and to superintend the education of his son. This kind offer Mr Hunter, after consulting his father, and his former instructor William Cullen, accepted.

Little doubt can be entertained, that this introduction to Dr Douglas was the foundation-stone, as it were, of the fortunes of William Hunter, and was an event pregnant with the most important consequences in the history of Anatomy and Anatomical Teaching in London. Hunter, when he first went to the metropolis, appears to have had no other object in view except that of acquiring such knowledge of anatomy and surgery as he already felt by experience he could not obtain in Edinburgh, and as might qualify him for practising with ease and confidence the duties of the surgeon and accoucheur. His friends had no other purpose in view; and, from a letter from his father on the proposition of Dr Douglas, it clearly appears that he was not only unprepared for, but opposed to, the plan of remaining in London, and had always expected to see his son settled as a medical practitioner in his native county, most probably at Hamilton, in company with Dr Cullen. William Hunter himself scarcely knew his own powers, and he evidently had no intention of becoming an authority in anatomy; and it was only when he had commenced the study of practical anatomy, with the knife in his hand, as he says, with the advice, instruction, and most likely the example of Dr Douglas, that he felt how much he could accomplish; that he saw the defects of the existing system, and what improvements he might introduce in the methods of teaching anatomy in London. This offer was made in July 1741, and appears to have been completed in August.

In this small history of occurrences apparently fortuitous, it has never been considered how providential it was, that William Hunter secured the friendship of a man like Dr Douglas. Had he not obtained the advice and guidance of some similarly qualified person, it is difficult to say how imperfectly developed his ideas might have been, how humble his position, and how limited the usefulness of his career. By becoming acquainted with Dr Douglas, and residing under his roof, it is impossible to doubt that Hunter's mind, original as it must be allowed to be, received an impulse in the proper direction, which was eventually of the highest consequence. Douglas had taken great interest in the improvement of the operation of lithotomy, and had laboured most usefully to make known its anatomical possibility. He was not only an anatomist, but a philosophical anatomist He had published, eleven years before the date of Hunter's appearance in London, the first good account of the distribution of the *peritonaeum*,—a short essay, which contains not only the views on the serous membranes long afterwards universally admitted among anatomists, and the earliest account of the separate existence and distribution of the adipose tissue, but many valuable observations on the diseases in which the peritonaeum is concerned.* This essay, which is now extremely rare, and is found in few public libraries, may be regarded as the precursor of that better known essay which William Hunter appended to his case of emphysema,† and as containing the germs of all that Hunter has said on the cellular membrane, the adipose membrane, and the serous membranes. Had Douglas written nothing but this single paper, that alone would have entitled him to a high rank among philosophical writers on anatomy; and though its merit has been obscured in the greater brilliancy of the essay by William Hunter and those by subsequent writers, as Andrew Bonn, Carmichael Smyth, and John Hunter, yet it is easy to see how great influence the intimate friendship of such a man must have exerted on the mind and faculties of Hunter.‡

Douglas was at this time residing in the Piazza, Covent Garden, to which accordingly William Hunter removed. He soon after attached himself to St George's Hospital, as a surgeon's pupil under Mr James Wilkie, and as a dissecting pupil under Dr Frank Nicholls. Of the means of instruction thus afforded, William Hunter availed himself with activity and perseverance. He soon became so expert in dissection, that Dr Douglas was at the expense of having several of his preparations engraved. In the course of not many months, however, death deprived him of this excellent and useful friend. Dr Douglas died on the 1st of April 1742, in the sixty-seventh year of his age, leaving a widow and two children.§

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* A Description of the *Peritonaeum* and of the *Membrana Cellularis*, which lies on the outside, with an account of the true situation of the ABDOMINAL VISCERA, in respect of these two membranes. By Dr James Douglas, Physician in Ordinary to his Majesty, &c. London, 1730. 4to.

† The History of an Emphysema. By William Hunter, M.D. Read Oct. 31, 1737. Medical Observations and Inquiries, Vol. ii. p. 17. London, 1762.

‡ James Douglas must not be confounded with his brother John Douglas, the surgeon, who rather disliked Cheselden.

§ It is melancholy to think that the son of this excellent man ruined himself, The death of Dr Douglas made no change in the situation of William Hunter, who continued to reside with the family of his late patron, and to pursue his studies with the same zeal and industry as formerly. This event, however, probably exercised considerable influence on the prospects and progress of Hunter; and it seems impossible to doubt that the loss of the friend so well qualified to advise, to guide, and to direct in the formation of plans, and to assist in their accomplishment, may have contributed to retard, though it certainly could not frustrate, the execution of those designs which Hunter had in view,

In 1743 William Hunter communicated to the Royal Society an Essay on the Structure and Diseases of Articulating Cartilages;* a performance which, though not quite free from error, and one of which, concerning the transition of blood-vessels between the cartilage and the bone, he afterwards rectified, shows the great attention he had been giving to minute structural anatomy, and its applications to pathology. He appears at this time to have entertained very strongly the idea of teaching anatomy, and to this object to have directed all his studies and devoted all his energies. The great means, however, were wanting, —an opening, and the support of some patron such as he had lost in Dr Douglas. Hunter communicated his intention to Dr Nicholls, who in 1745 gave up lecturing in favour of Dr Lawrence; but Nicholls gave no encouragement; and the plan of Hunter might have been either frustrated, or deferred so long that it must have been less successful than it eventually proved, had not an accidental circumstance taken place to give him the opportunity he desired.

We have said that there was at that time little systematic dissection in London, and few means of studying anatomy in a consecutive and systematic manner,—still fewer of studying the applications to surgery. Dr Hunter himself long afterwards stated that, "in the only course which he attended in London, which was by far the most respectable that was given in that city, the professor, Dr Nicholls, used only two dead bodies in his course." The course also was finished in from thirty to forty lectures; and, consequently, he adds, it was contracted into too small a compass of time, and therefore several material parts of anatomy were left out entirely. Mr Bromfield, who was surgeon to St George's

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it is said, by indiscretion, and died at the age of thirty, about the year 1755. This circumstance has been by some believed to be a reflection on the gratitude of William Hunter, that he allowed the son of his earliest patron in London to die unaided and destitute. This is not the fact. Hunter repeatedly aided him as far as his own means allowed; and at the period of Mr Douglas's death, Hunter held promissory-notes of his to the amount of L.100,—a sum then equivalent to fully L.200 of the present day. But Mr Douglas was unfortunately a man to whom all aid was unavailing.

* Philosophical Transactions, vol. xlii.

Hospital, comprised Anatomy and Surgery in a course of thirty-six lectures; and Mr Nourse of St Bartholomew's embraced his entire course in twenty-three lectures. "Let those who can, reflect," says Dr Hunter, "how imperfect such courses were; let them recollect that they never saw the human brain and nerves, nor the human lymphatic system, nor the human gravid uterus and its contents; let them recollect that there were few parts of the body which they understood thoroughly, after attending even repeated courses of that kind; and then they will see that there was great room for improvement, and own that an opportunity of attending a more complete course of anatomical lectures must be a national advantage."* Hunter even went so far as to assert that before his time there was no school of practical anatomy in London, and that he was convinced, from feeling the advantage of dissecting himself, and examining the different parts of the body at leisure and in his own way, he might be of considerable service to the public as a teacher by opening a school for that purpose; and he maintained that the influence which this proceeding had in this country is evident.

There seems to be no doubt of the fact that the Great Metropolis furnished at that time no efficient school,—none in which students could have opportunities of examining the organs of the human body so frequently and so carefully as to make them either expert anatomists or dexterous surgeons. Such was the state of anatomical teaching when an accidental occurrence made William Hunter a lecturer.

A society of naval surgeons, desirous, as opportunity afforded, of maintaining and extending their professional knowledge, kept an apartment in Covent Garden, where they engaged Mr Samuel Sharpe, then surgeon to Guy's Hospital, to deliver a short course of lectures on the operations of surgery. This course Mr Sharpe continued to deliver for several years, till the winter of 1746, when his engagements having increased so much as to prevent him from performing the duty in a satisfactory manner, he declined it in favour of Mr Hunter, who performed it with so much ability, that the Society requested him to extend his plan, so as to deliver a course of lectures on anatomy. This was the first course of public lectures on that subject which William Hunter delivered;† and he appears to have continued for at least eleven or twelve years to lecture in the same place. We find, at all events, that in 1756, ten years after the first course of lectures, he was still teaching in Covent-Garden,‡ though now with the addition of the assistance of his brother, John Hunter; and here he remained till 1762, when we find him delivering lectures with Mr Hewson in a theatre in Litchfield Street, where the school was continued till 1769, when the apartments in Windmill Street were completed. Dr Hunter during this period gave two courses in the lapse of the winter. One course,—the autumn one,—was commenced in the beginning of October, and ended before New Year's Day. The second, called the spring course, used to begin about the 20th of January, and terminated some time about the 20th of April. Each lecture occupied two hours; so that these courses were not only the most complete in London, but were quite equivalent in duration to the five months and a half courses of the Edinburgh College.

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* Two Introductory Lectures delivered by Dr William Hunter to his last course of Anatomical Lectures, &c. London, 1784. 4to, p. 100.

† "Mr Symons of Exeter, surgeon, attended my first course of lectures in 1746 and the three following courses, and likewise assisted me in dissections."—Medical Commentaries, Part I. p. 8. Lond. 1762.

‡ "One morning, some time in the autumnal course of lectures of 1756, Mr

The step thus taken by William Hunter was one which implied not only confidence in his own talents and powers, but a large amount of courage, in attempting what was in a great degree unprecedented. Hitherto all teaching of anatomy had been conducted only by Fellows of the College of Physicians, to whom, by prescriptive right, the College of Surgeons delegated this power; or by hospital surgeons in connection with one of the large hospitals. Among the former class of public teachers were such men as Harvey, Glisson, Willis, Mead, Lower, James Keill, Dr James Drake, Dr Wagstaffe, Francis Nicholls, and Lawrence, all of whom were appointed by the College of Surgeons as Readers on anatomy,—a name adopted from the practice of the English Universities. Cowper and Cheselden were the first who as surgeons held this office; and after the example set by them, it was followed by Sharpe, Bromfield, Nourse, Henry Watson, and others more recent. William Hunter, when he commenced the business of teaching anatomy at the age of twenty-eight, was not only unconnected with any hospital, but had no prospect of being so connected, at least in the capacity of surgeon. Neither could he have any prospect of becoming connected as fellow with the College of Physicians; for this the want of university education at Oxford or Cambridge completely and absolutely precluded. He commenced, in short, in all respects as an unauthorized and unsanctioned teacher; and his instructions must have been attended solely for the value of the information which they communicated.

His success, nevertheless, was almost immediate and continuous. The profits of his two first courses, delivered in 1746 and 1747, are said to have been considerable. Yet, by contributing to the

Pott called upon me (John Hunter) in Covent Garden."—Med. Commentaries, Supplement, Lond. 1764, p. 13. "The professor (Dr Monro) called at my house in Jermyn Street, 13th December 1757, when he knew I was in my lecture-room in Covent Garden; and Mr Pott called at these rooms in the morning, because he knew I was there only in the evening."—Supplement, p. 18.

wants of various friends, he found himself obliged, at the return of the ensuing session in October 1747, to defer commencing for a fortnight, in consequence of wanting money to defray the necessary expense of advertisement. This circumstance made him more cautious in future.

Meanwhile he had become, on the 9th August 1747, a member of the Corporation of Surgeons, intending to practise Surgery and Midwifery. The spring and summer of 1748 he spent in a tour through Holland, Flanders, and part of France. Among other eminent persons he visited Albinus at Leyden, and was inspired, by the magnificent preparations shown him by the Dutch anatomist, to imitate his example in that department of the anatomical arts.

From a letter addressed on the 20th September to Dr Cullen, it appears, that, previous to this time, he had returned to London, ready to resume his duties as lecturer. Meanwhile, an event took place which added much to the stability and note of Hunter's rising school, and has contributed more than perhaps could have been anticipated, from a circumstance apparently so ordinary, to render it a place of distinction. His brother, John Hunter, then a young man of twenty, after spending some time in a desultory manner at Glasgow with a relative, turned his attention to London in imitation of William Hunter, and having received from him every encouragement to come to the metropolis, set out the same month (September) on horseback, then the usual mode of travelling, and arrived in London about two weeks before the commencement of his brother's lectures. John Hunter began forthwith to study practical anatomy, and dissect industriously under the inspection of Mr Symonds, his brother's assistant; and such good use did he make of his time, that the subsequent season he was able to assist in instructing the pupils in the dissecting rooms. To William Hunter the presence of his brother John was a great acquisition; for though, from the peculiar temper of the latter, and the unyielding character of both, it is to be apprehended that they did not always go on in the most cordial manner, and eventually quarrelled, so as never to be reconciled, still William evidently appreciated highly the vigorous and active mind of John Hunter, and was also proud of him; and there cannot be a doubt that the character of John, speedily known as a bold and original thinker, added greatly to the reputation and influence of the school of William Hunter. John Hunter, indeed, it is superfluous to observe, proved, if not the first, certainly not the least distinguished among a goodly number of able anatomists and physiologists, bred under the auspices of this active and enthusiastic teacher. He continued for ten years to conduct the labours of the dissecting rooms, investigating various important and undetermined subjects in anatomy and physiology, at first in the human body, and afterwards in the bodies of animals in general. These labours produced an injurious impression on his health, and obliged him, in 1759, to relinquish the scene in which they were conducted, and, after making some temporary recovery from a serious disorder of the lungs, with which he was threatened, to seek professional employment in some way less injurious to his general health. This led to his being engaged as staff-surgeon in the public service in March 1761, and gave him an opportunity of studying the effects of gun-shot and other wounds on the large scale, during the expedition to Belleisle on the west coast of France. From this military expedition he returned in 1763, but he did not again join the establishment of his brother.

In the same year, 1748, William Hunter was elected one of the surgeon-accoucheurs to the Middlesex Hospital, and in 1749 to the British Lying-in-Hospital in Brownlow Street.

In October 1750, William Hunter obtained from the University of Glasgow the degree of Doctor in Medicine, relinquished the practice of surgery, and confined himself to that of physician and physician-accoucheur. At the same time, Dr Hunter quitted the family of Mrs Douglas, and went to reside in Jermyn Street.

William Hunter may now be considered as having established completely his plan of teaching anatomy by complete courses of lectures and by dissections, on a scale much more extended than had yet been seen in any town in this country, or perhaps anywhere on the continent; for it does not appear that either in Paris or Bologna was the same amount of practical anatomy taught. This, however, he could not have accomplished without the active aid of his brother John, and the other gentlemen assisting more or less constantly. William Hunter was indeed beginning to be occupied so much either in accoucheur practice or other professional engagements, that though his zeal prevented him from deserting the anatomical theatre, yet he was often withdrawn from its duties by others not less imperative. This constant occupation and demand on his time is the main reason why, with all his means, William Hunter has written and published so little.

Excepting the Medical Commentaries to which he was urged by the jealous dread of being deprived of the merit of some of his discoveries, two Introductory Lectures, several detached papers in the Philosophical Transactions and the Medical Observations and Inquiries, the only written remains of the long and active career of William Hunter is the work on the Gravid Uterus,—a work accurate, splendid, and useful indeed, if we look to the labour expended on it and the information communicated, yet small compared to what its talented author might have accomplished.

It is not, however, in the immediate productions and writings of a man like William Hunter, that we can expect to find all the results of his exertions, or that we see the fruits of all his studies and investigations. He became the founder of a school for teaching anatomy and physiology. He had the penetration to see and feel what was defective, and he possessed courage and originality to devise and carry into effect a new and greatly more efficient system, which, simple as it was, had not previously been thought of by any of the able and accomplished men, who had preceded him. He may be said to have realized in the medical sciences some of those schemes which Bacon had excogitated and proposed as to philosophy. Without aid or patronage, he established, for training young physicians and surgeons in the fundamental and most necessary elements of scientific medicine and surgery, a school which gave birth to several of the most able men of the eighteenth century, which, but for the barbarous illiberality of a niggardly and selfish government, might have rendered more honour and done more good to the country than both the Universities, and the name of which, amidst all these difficulties, still lives in the great scientific works which were achieved by its pupils.

One of these pupils, we have seen, was John Hunter, a name which must endure as long as physiology is taught and surgery practised among the human race. But other three followed, two of whom were scarcely less celebrated, and not much less useful. These four disciples of William Hunter laboured to extend and establish, on a solid foundation, the doctrine of the pre-eminence of the lymphatic vessels as a system of absorption, to which their master was so devotedly attached; and while his brother experimented, the other three employed most assiduously all the practical means in their power to prove the great extent of this system in the animal body and in the animal world, to elucidate every point relating to its anatomical history and distribution, and to establish on an irrefragable basis their own claims to distinction amidst the apathy and ignorance of a selfish generation. The controversy regarding the absorbing function of the lymphatic vessels, which has in our days fallen to its just level, was then viewed as one of the greatest importance in physiology. It was connected by means of the lacteals with various important points regarding the function of digestion and chylification; and while the task of proving that these vessels absorb, was regarded as next, if not equivalent to that of the circulation of blood by the arteries and veins, it necessarily excited all the talent and commanded all the resources of several ingenious and enthusiastic men; while it attracted more or less forcibly the attention and interest of numerous followers and observers. Some adopted and eagerly defended the doctrine; others thought it probable; others fluctuated between doubt and certainty; a few were indifferent, and probably scarcely understood the questions at issue, or the means by which they were to be determined. But all who wished to be thought scientific, agreed in admiring, or professing to admire, the zeal, energy, and ingenuity of William Hunter and his disciples. This ferment of intellect and practical skill produced, within the space of about fourteen years, three treatises on the lymphatic and absorbent system, all by the disciples of Hunter; treatises altogether new in the history of English anatomy, and so much the more highly to be appreciated, that their publication never could have remunerated their authors for their time, labour, and pecuniary outlay expended in preparing them. The ferment has subsided; but the works remain, monuments of the devoted and disinterested energy of their authors, and showing how much real good may be achieved by the sincere love of even an hypothesis.

It is not without interest to observe the manner in which William Hunter himself speaks of this succession of able men, and their labours in elucidating the anatomy and uses of the lymphatic system.

"Accordingly, my brother, Mr John Hunter, whom I bred to practical anatomy, and who worked for me, attended my dissecting-room, and read some lectures for me many years, found some lymphatics first in birds, and then in a crocodile."

"Next, Mr Hewson, whom I first bred to anatomy, and then took into my house to work for me and under my direction in Practical Anatomy, to attend my dissecting-room, and read some lectures as my partner, which he did for a number of years; by a continued course of observations and experiments made in this house,* discovered and fully demonstrated the lymphatics and lacteals both in birds and fishes, which confirmed the use and importance of the absorbent system in the human body; and, in Comparative Anatomy, was one of the greatest improvements that could have been made to establish the universality of nature's laws in animal bodies."

"And, last of all, Mr Cruikshank, whom I likewise bred to anatomy, and took into my house on the same plan, with the opportunities which he has had in this place, and by being particularly attentive to the lymphatic system, at my desire has traced the ramifications of that system in almost every part of the body; and from his dissections, figures have been made, which, with what I had before, will enable us to publish, we hope, in a little time, a full account of the whole system, illustrated by accurate engravings."†

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* In Dr Hunter's dissecting-room in Windmill Street.

† Two Introductory Lectures by Dr William Hunter, &c. pp. 60 and 61. London, 1784. 4to.

It thus appears that William Hunter was the great moving principle and animating spirit, as it were, of all this system of dissecting, injecting, and making preparations.

Many a man, we have no doubt, now says: The Absorbents, the Lymphatics, what is the use of them? what is the use of studying them? It has been proved that they are not the sole agents of absorption. It has been shown that absorption does [illegible] take place in the animal body. What is it to me in what manner or by what agents it is effected? I know that absorption takes place; and John Hunter, Hewson, Cruikshank, Sheldon, Abernethy, Cooper, Magendie, Dupuytren, Andral, Chomel, and Louis, knew no more. With that knowledge I can prescribe medicines, and cure diseases; and all the dissections and experiments of the Windmill Street school, or any one more recent, cannot enable me to do more. All this is, in one sense, true. Yet thus reasoned not the followers of Hunter; nor will any man do so, who loves knowledge for its own sake. It is to be observed, that the results of knowledge may be negative as well as positive. The philosophical and rational mind does not regulate its inquiries by this simple utilitarianism. It is, if not necessary, at least very desirable and agreeable, to extend them beyond these limits, if it were for no other purpose than knowing that we know all, and that nothing else can be known. Considerations of this kind it was, probably, which led, first, Hewson to study and make known his researches in the lymphatic system; then Cruikshank after him; next Sheldon; and, finally, Mascagni, in Italy, when this subject seemed to be so thoroughly made known, that scarcely any human mind could be expected to throw new light on it, or add one new fact, or discover one more conclusive argument. New arguments, nevertheless, were added. New views were taken; and now, perhaps, at the distance of eighty years, when the nineteenth century is nearly half finished, we think that our views are the just and natural, and that we rectified the mistakes of our predecessors, and reduced to their just dimensions all the extreme doctrines of those who have previously dogmatized and experimented on this subject. All that can be said is, time, the great judge, will show; and to that arbitration the decision of such questions must be left.

Yet, amidst the fluctuations of physiological doctrine, and the variations of medical theory, the value of the anatomical labours on which they rest ought to remain more or less undiminished. Among the great names, indeed, by which the history of anatomical science in the eighteenth century is adorned, none are entitled to higher respect, or claim a greater degree of veneration, than those of William Hewson, William Cruikshank, and Paul Mascagni. All three studied most industriously the lymphatic system, and not only extended our knowledge of that system, but rendered that knowledge precise and accurate. All of them produced by their personal researches Treatises, each of which alone might have been sufficient to illustrate the history of that branch of anatomy. And each of them, by these personal researches, added so many new and accurate facts, and left the subject so thoroughly explored, that almost nothing remained to be done by succeeding anatomists.

The latter circumstance is one principal reason that, while to the English anatomists a third name ought to be added,—that of John Sheldon,—yet his services, great as they unquestionably are, have been obscured, if not lost, in the lustre which has surrounded the great names of his two countrymen.

Of the four works indeed published within the space of twelve years,—that is, between 1774, when Hewson's small but accurate treatise appeared, and 1787, which witnessed the publication of the gigantic and elaborate work of Mascagni,—the last was certainly the most ample and complete. Yet coming, as it did, after the works of Hewson and Cruikshank, it was not held so original or so novel, and never probably so generally and carefully studied as the performances of the two English anatomists.

Of all these great names, the most distinguished in many respects, as he was likewise the first, was William Hewson; and this he owed more certainly to the vigour of his genius alone than to any other circumstance. Hewson, Cruikshank, and Sheldon, were all pupils of William Hunter; all shared the advantage of being under that industrious, energetic, and zealous instructor; and all probably shared in the impulse which the intelligence and spirit of so powerful a mind doubtless communicated to every one who came within the sphere of its action. But Hewson appears himself throughout to have been a person of that powerful intellect and active mind that must in every situation have thought and acted for himself, and who was quite capable of extending the boundaries of any science or any department of science which he undertook to cultivate. The reasonings and experiments of Hewson show everywhere the operation of a mind at once original, clear-sighted, and precise. His descriptions give exactly what he wishes to communicate, what the subject requires for elucidation, and what the student desires to know, and no more. He begins with the most simple and elementary facts and statements, and gradually leads his reader to the most complex. All his corollaries and inferences are stated by themselves in clear, intelligible, and philosophical language; neither confused nor confident, yet in such a manner that they make a deeper and more durable impression than if they had been enunciated in a more decided tone.

With Cruikshank, on the other hand, it is altogether different. He communicates, indeed, a great amount of information on the anatomical distribution of the lymphatic system, so much, indeed, and so well, that if his were the only book on the subject, it would be thought the most perfect that could be written. The book also reads marvellously well the first, the second, and perhaps the third time, because he applies his facts with great ingenuity to explain and correct many isolated phenomena in physiology and pathology. Yet, when we come to examine it logically, we find that the excellent author often sins grievously against method and order. A most interesting and valuable observation is often found in a situation where it was least expected. It makes a strong impression at the time; and the reader retains this impression, till some subsequent period when he wishes to recal it. He cannot again find the passage, or the fact which it contains, solely because it is not in the place in which he thought it right that it should be; and when, after a tedious and laborious search, he comes on the pearl, he finds it not indeed among rubbish, but very oddly mis-set.

Numberless examples of this occur in the two works published by Cruikshank. Correct anatomical statements and descriptions stand amidst interesting physiological corollaries, and instructive and original pathological remarks. All are striking; all are useful. But they are arranged, or rather disarranged, in a manner so singular and heterogeneous, that it is difficult to perceive sometimes the chain by which they are kept connected. How original and just are his observations on the different forms or rather pathological causes of dropsy? How important the remark that this morbid effusion may be the result of inflammation?* How pregnant with important inferences is the fact which he casually states, that he found attached to the origin of the pulmonary artery in a boy of nine years a laminated coagulum of blood, about the size of a small cherry, nearly blocking up the cavity of the vessel? Again, he had observed in the right auricle of the heart of a man of 35, who was melancholy, and thought himself emasculated, while he had great dyspnœa, an abscess, containing about one ounce of purulent matter, and surrounded with coagulated laminated blood to a considerable thickness. And all these heterogeneous yet valuable facts he puts down, with the intention of illustrating the doctrine of absorption, but often with the effect of refuting it,—assuredly not confirming its truths,— and causing infinite confusion in all his reasonings and inferences. Then lymphatic absorption was in his hands to accomplish everything; to cause diseases and to cure them; to cause the absorption of poisons of every kind,—atmospheric, terrestrial, animal, and vegetable; to explain the mode in which morbid poisons may be prevented from operating, and how long a person bitten by a rattlesnake or a cobra de capello could live; why inoculation for small-pox failed in some cases, and succeeded in others; why it acted not, while the poison of measles was latent in the system, and showed itself only after that of measles had exhausted its effects. Lastly, to wind up all properly, he enunciates such philosophical truisms as the following: "That the human species were not intended to live for ever; and that death is not an evil. Nature has not only provided for our existence and duration for a certain period, but also for our dissolution." One would have thought he had anticipated the era of the homœopathists and hydropathists, and similar believers in the invariable operation of all remedies, the curability of all diseases, and the immortality of the human race.

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* "A man receives a blow on the testicle; inflammation takes place, and the consequence is frequently a hydrocele or dropsy of the *tunica vaginalis.* A child's brain inflames, and this inflammation ends at last in *hydrocephalus*, or collection of water in the brain. Pleurisy frequently terminates in hydrothorax, or collection of water in the chest. I have often taken away forty or sixty pints of water, which had accumulated in the cavity of the abdomen in the few days the peritoneal inflammation had lasted during the usual species of child-bed fever. This is to be considered the substituting a less dangerous disease for another. Peritoneal inflammation kills often in three days; but askites may last twenty years."—The Anatomy of the Absorbing Vessels, second edition, London, 1790, p 116.

The Anatomy of the Absorbing Vessels is, with all these and many more faults, a most valuable and instructive book; a book the merits of which cannot be fully perceived on a first, a second, or a third perusal. It contains facts which instruct and edify at the twentieth time; facts which seem always new and previously unknown; and which are so much the more valuable, that, while they are evidently deduced from careful observation, they contain the germs of many modern ideas in physiology, and especially pathology. He was evidently quite familiar with *melanosis*, and the calcareous transformation of lymphatic glands, with the formation of tubercular matter in the lungs and in glands, with the formation of skirrhus, and many other growths and degenerations, which, in recent times, we have seen more fully and elaborately, but scarcely with more accuracy, described.

Of still higher value, perhaps, though in a different way, are the writings of William Hewson. We are not, indeed, in them astonished with the same number of precious and useful pathological and practical remarks. But we find whatever is communicated, arranged in more lucid order, and expressed with greater precision. The essays on the lymphatic system by Hewson, though published at an earlier period, are nevertheless quite as complete; and, on perusing them, the reader thinks he obtains even a more distinct and comprehensive view than that which is given by his two successors. Hewson, indeed, by giving a view of the lymphatic system not only in man, but as it is found in birds, reptiles, and fishes, presents a more ample and instructive delineation on the whole than Cruikshank, who confined his description chiefly to the lymphatic vessels of the human body. But we meet not with the varied and instructive, though desultory observations on diseases, with which the work of Cruikshank is at once diversified and rendered useful.

Hewson, however, confined not his researches to the lymphatic system. He was the first who examined microscopically the blood and its globules, and studied to unfold the mysterious properties of that liquid. The Experimental Inquiry into the properties of the blood, first published in 1771, contains still one of the most accurate accounts of the mechanical and anatomical constitution of that liquid; and though multiplied researches on it by many different observers have since that time been made, and the microscope has been greatly improved, the performance of Hewson is still read with interest and advantage. This subject Hewson continued to investigate to the last, convinced, apparently, that the information to be thus acquired, was to throw great light on several of the most important processes and functions of the living body.

We must not, however, commit the error which we reprobate in others; and it is necessary that we give some account of the short though active life of Hewson, and his connection with the school of William Hunter.

William Hewson was born at the ancient town of Hexham in Northumberland, on the 14th of November, O. S., 1739. The rudiments of his education he received at the grammar school of that town, under the Rev. Mr Brown. His mother, whose maiden name was Heron, was a native of Hexham, and allied to several respectable families in the place. His father was a surgeon and apothecary in Hexham, much respected in the neighbourhood; and with him, William Hewson acquired his first medical knowledge. Being ambitious to increase this knowledge, he placed himself for some time under Mr Lambert, an eminent surgeon at Newcastle, where opportunities of general and surgical practice were more numerous than in a town like Hexham.

In the autumn of 1759, when he was twenty years of age, William Hewson came to London, resided with John Hunter, and attended the lectures of Dr William Hunter in his rooms in Covent Garden. His assiduity and skill soon recommended him to the favourable notice of the brothers; and when, in the end of March 1761, John Hunter went abroad with the army, to Mr Hewson was committed the duty of instructing the pupils in the dissecting-room. Meanwhile he became a pupil at Guy's Hospital and at St Thomas' Hospital, and attended the lectures of Dr Hugh Smith on the practice of medicine, and those of Dr Colin Mackenzie on midwifery.

In the autumn of 1761, he went to Edinburgh with recommendatory letters from Dr William Hunter and Sir John Pringle, to the professors at the University of that city, and studied there, according to his own account, the latter part of 1761, and the beginning of 1762, and, according to Mr Gulliver, till the winter of 1762. Probably the autumn is the time understood; for Hewson, it is said, returned to London in 1762, in time for the lectures given by Hunter, entered into a partnership with him, and delivered part of the lectures, receiving at the same time a proportion of the proceeds. Their anatomical school, it is stated by Mr Gulliver, was in Litchfield Street, where Mr Hewson resided, and where some of the pupils lived under his roof.

At Edinburgh he appears to have attended particularly the lectures of Dr Monro, and to have imbibed some of the rather strong feelings of William Hunter against that gentleman. On thinking on the controversial correspondence which afterwards arose between Dr Monro and Mr Hewson, it appears to be almost the only passage in the otherwise peaceable life, which he lived, that the true lover of science would wish had been otherwise.

In the summer of 1765, Mr Hewson went to France, spent some time in Paris, and, returning through Flanders and Holland, arrived in London before the commencement of the autumnal course of lectures.

Hewson had already been studying comparative anatomy with great zeal and assiduity; and it is manifest that his pursuits received a decided impulse from the known anxiety of William Hunter to elucidate and confirm his favourite doctrine of the absorbing function of the lymphatic vessels. Hunter, having adopted strongly this idea, according to his own account, so early as 1741, availed himself of every opportunity of illustrating, proving, and enforcing it. In 1758 and 1759, John Hunter performed those experiments, which were believed irrefragably to demonstrate the absorbing function of the lymphatics, and the incapability of the veins to do so. And Hewson, introduced into a position in which the importance of this question was repeatedly and strongly placed before him, began in no long time to collect, from the anatomical distribution of the lymphatics, facts which might contribute to strengthen the doctrines of his instructor. It is manifest that with this view he spent part of the summer of 1768 on the coast of Sussex, making experiments upon fish; he had already done much as to Birds and Reptiles in London; and on the 3d of October his Account of the Lymphatic System of Birds was communicated to the Royal Society, and read on the 8th of December of the same year. The ensuing year his Account of the Lymphatic System in Amphibious Animals, and that of the Lymphatic System in Fishes, were both presented on the 19th of June, and read respectively on the 9th and 16th of November. This was the first form in which the description of the lymphatic system in the human subject and in other animals appeared.

In 1769 the building erected by William Hunter in Great Windmill Street was finished. A small apartment was assigned to Hewson, who not only continued to assist in the rooms, but delivered a larger proportion of the lectures than previously. At the same time it appears that, without effort on his part to obtain practice, employment both in surgery and midwifery came upon him ultroneously, and added considerably to the amount of his revenues.

Mr Hewson had, in 1768, become acquainted with Miss Mary Stevenson, daughter of Mrs Stevenson, residing at Craven Street, Strand, a lady who is represented by all that knew her to have possessed great judgment, considerable mental accomplishments, and an amiable temper. Miss Stevenson had been known from her 18th year (1757), to the celebrated Dr Franklin, who, in his visits to London, was wont to reside in the house of Mrs Stevenson. This lady Hewson married on the 10th of July 1770, and shortly after he took a house near Dr Hunter in Jermyn Street.

The partnership was, however, dissolved in 1770, in consequence of some misunderstanding not fully explained, but relating apparently to the right of Mr Hewson making anatomical preparations for himself, when he was still connected with Dr Hunter. Mr Hewson then began, on 30th September 1772, to deliver lectures, on his own account, in a theatre which he had erected in Craven Street, adjoining a house which he intended for the residence of his family. Before beginning this course of lectures, he delivered, before a respectable assemblage of men of science, a discourse on the Uses of the Spleen and Thymus Gland, a subject on which he had already been forming various ingenious and new opinions and conjectures. The course of lectures which followed was very favourably received, and attended by more than half the number of pupils who listened to the combined instructions of Dr William Hunter and himself. The second course in the spring, as well as those of the subsequent season, were equally successful.

Thus Dr Hunter not only himself established a school for teaching anatomy independent of the hospital schools, but he was the means of another teacher of decided talent forming a similar institution, which gave every promise of being equally successful and equally beneficial to the profession.

In May 1770, he had communicated to the Royal Society, in two separate memoirs, the Experiments on the Blood, and Remarks on its Morbid Appearances; On the Degree of Heat which Coagulates the Lymph and the Serum, with an Inquiry into the Causes of the Inflammatory Crust; in July, Further Remarks on the Properties of the Coagulable Lymph, on Stopping Hemorrhagies, and on the Effects of Cold on the Blood; and in June 1773, an Essay on the Figure and Composition of the Red Particles of the Blood.

So great a number of interesting memoirs, on subjects new, difficult, and complicated, the materials for which must have been collected amidst the labours of teaching and the distractions of professional occupations, indicate the possession of a mind vigorous and original in no ordinary degree, and gave promise of a career as brilliant as it must no doubt have been useful. The labour of writing, and recording, and arranging facts, where all of them must be derived from repeated careful observation, is great, and the progress in general slow and tedious. Yet, in the space of not more than five years, Hewson had found means to communicate to the first scientific society in the kingdom the whole of a New and Correct View of the Anatomy and Physiology of the Lymphatic System, and to create, as it were, from careful microscopical observation, an entire new history of the blood, and its phenomena in the living body. He must have employed, in scientific researches, every hour and day of his time, not occupied by professional engagements; and to any one who knows from experience how slow is the progress, in the most favourable circumstances, in such undertakings, it will appear almost marvellous, that, in so short a space of time, he had accomplished so much.

His fame was indeed established. His public lectures at Craven Street not only were attended by eager crowds of zealous and attentive students, but by many practitioners well up in life, who were anxious to learn from the teacher himself the new and useful truths which he had been able to ascertain. It is painful to think on the precarious nature of the most useful forms of human exertion, and of the multiplied perils by which the prosperity of the medical labourer may be subverted. A wound received in dissection, towards the end of April 1774, was followed by the usual local effects and febrile symptoms; and on the first day of May, William Hewson died, in the thirty-fifth year of his age.

The separate essays on the blood had been collected, arranged, and published in one volume in 1771; a second edition of this, under the title of EXPERIMENTAL INQUIRIES, Part I., was published in 1772; and the latter appeared as a third edition several years after the death of the author, in 1780; forming, ultimately, the first volume of the collected writings of William Hewson.

The denomination EXPERIMENTAL INQUIRIES had been originally chosen by Hewson himself in 1772, as a general title to the whole series of his Researches. Of these the Memoirs on the Lymphatic System in Man, Birds, and Reptiles, now collected in another volume, formed the second part. This appeared in 1774, previous to the death of the author.

The third portion of these Experimental Inquiries, which contained all his other experiments and reasonings on the Red Particles of the Blood, some observations on the Structure of the Lymphatic Glands, and the Discourse on the Structure, Situation, and probable Uses of the Thymus Gland and Spleen, was given to the public after his death in the year 1777, by the care of his assistant and brother-in-law, Mr Magnus Falconar. It is proper to mention that, excepting the first chapter of this third part, which is the paper communicated to the Royal Society in June 1773, none was actually written by Hewson. But Mr Falconar, who had been on the most intimate terms with him, and was perfectly familiar with all his views, and had means of knowing with great accuracy the facts on which these views were founded, completed from his personal knowledge the volume, as nearly as it could be conjectured, in the manner in which it would have been done by the author himself. This amounts to four chapters. Mr Gulliver thinks that, though Falconar has performed his part of the duty with great care and fidelity, yet Hewson would have modified some of the statements had he seen them.

Mr Falconar, who was a native of Cheltenham, and had married the sister of Mr Hewson, did not long survive his relative and instructor. In the course of three years he began to present symptoms of pulmonary consumption, and died of that malady at Bristol, on the 24th of March 1778, at the age of twenty-four.*

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* There is some discrepancy, which we cannot attempt to correct or reconcile, in the statement regarding the date of Mr Falconar's death. In the life of William Hunter by Dr Foart Simmons, the statement is as we have now given it; and this we see is also given from the same authority by Mr Gulliver, in a note on the life of Mr Hewson, at page xlvi. In the note marked cxx. however, in the third part of the Experimental Inquiries, at page 251, Mr Gulliver expresses himself as if the date of the death of Mr Falconar were a point not ascertained. "I apprehend Mr Falconar did not live to fulfil his promise, although he devoted some space to the subject in the Synopsis of his Lectures, dated from Craven Street, Strand, London, Sept. 26, 1777. He must died within a month afterwards; for the sale catalogue of the museum formed by the joint labours of these two young anatomists, is dated in the following 12th of October." Is this the 12th of October 1777 or 1778? If the former, then the museum must either have been sold before the death of Mr Falconar, or he must have died, as Mr Gulliver thinks in this note, between the 26th of September and the 12th of October. On the other hand, it is distinctly stated by Dr F. Simmons, who was in general an accurate person, that Mr Falconar died on the 24th of March 1778; and it may have been the case that the museum was sold during the life of Mr Falconar, when the circumstance of his labouring under a disease most likely to end fatally, shut out the prospect of his either teaching that winter, or in any way using the museum. After all, the doubt expressed in this note (cxx.) may have originated with Mr Gulliver.

The sale of his anatomical collection, which included that of Mr Hewson, produced upwards of nine hundred pounds,—a very large sum in those days, and shows how valuable it must have been esteemed. The greater part of it was bought by William Hunter.

William Hewson's mortal remains were interred on the 6th of May 1774, at St Martin's-in-the-Fields; and the only record of the event is contained in the Funeral Register kept in the church, with the simple words, " May 6, 1774, William Hewson, a man." Neither the spot containing his remains nor the event itself are indicated by the existence of any epitaph, monument, or memorial of any kind. His grave Mr Gulliver was unable to find.

But though Hewson was thus allowed to descend into a grave unknown and undistinguished, it was impossible that his name and the fame of his labours should be equally concealed. It was felt indeed by all intelligent members of the profession, and by many men eminent in science, that a great and highly-gifted physiologist had been cut off in midtime of his days; and that, so far as human sagacity could judge, the anatomical sciences had sustained a loss in the meantime irreparable. Dr Lettsome pronounced a just and affectionate panegyric on the talents of the deceased lecturer; and his name was long familiarly remembered in the medical schools of London as the pupil, colleague, and finally a sort of rival teacher to William Hunter, and as the first who explored very accurately and fully the remote recesses of the lymphatic system. On the Continent, his character as an anatomical and physiological writer was, if possible, still more highly esteemed; and his works were translated into Latin by Professor Hahn, and thus made generally accessible to foreign scholars, a compliment at that time paid to few.

The reputation of Hewson was not, like that of many other celebrated persons, great during his life, and more or less speedily dwindling away after death. It has continued progressively and steadily to increase, and promises not to be speedily diminished. This must be ascribed to the actual value of his writings, and not in the slightest degree to any adventitious or accidental circumstance. To these writings we have already adverted in the order of their appearance; and, after his death, and that of his brother-in-law, no second collected edition appeared. From this circumstance,—which is partly to be ascribed to the fact, that they are scientific, and not of practical character, and that no profession takes less interest in scientific works than the members of the medical profession,—the writings of Hewson had become exceedingly scarce. No complete copy was to be found in any bookselling establishment, and even in several public libraries they were not to be found. It is indeed only at the sales of old libraries that for a long time past either his works or the treatise of Cruikshank could be ever obtained. As works of this kind are read by few and asked for by fewer, no bookseller would undertake the preparation of a new edition. But as, on the other hand, amidst the great body of unreading and uninquiring professional men, a few are still found who take interest in perusing the writings of Hewson, and comparing the facts ascertained by his industry with those subsequently brought to light, it seemed that it was proper to provide even for this small section the means of gratifying a rational and laudable curiosity. This duty the Sydenham Society have undertaken; and to carry their plans into effect, they selected a gentleman remarkably well qualified for the purpose,—both by his knowledge of the history of anatomy and physiology, his ability as a microscopical observer, and his love of science and its cultivators,—and who has accomplished his task in the most creditable manner. The present complete edition of the collected works of William Hewson is introduced to the reader by an interesting biographical sketch of the author, and an account of his writings, which Mr Gulliver has rendered more instructive by a view of the history of physiological doctrine on the spontaneously coagulable principle of the blood. In the arrangement of the materials of the volume, the same order as that observed in the Experimental Inquiries is followed; and in order to carry forward the progress of scientific research to the present time, Mr Gulliver has illustrated the text by the addition of a complete series of notes, containing information of the most accurate nature from all the subsequent authentic sources. These notes amount in number to one hundred and fifty-six, and they bear honourable testimony to the information and industry of the Editor.

We have some doubts whether this mode of arrangement be the best; and whether, had the decision been left to one who looked on method and unity as the first requisites in a literary performance, this be the one which ought to be adopted. The arrangement of the old edition, which is chronological, and therefore not to be lightly abandoned, was that which the circumstances and mode of publication in some sense compelled Mr Hewson to adopt. From the same circumstances, however, it resulted that, in adopting this method of arrangement, the unity of the subjects was grievously violated. Thus, the first part of the Experimental Inquiries on the Properties of the Blood, as it now stands in the third edition of that part, is concluded by, or has attached to it, the controversial Appendix relating to the Discovery of the Lacteal System in Birds, Fish, and the Animals called Amphibious, that is, the Reptiles, and vindicating the right of Mr Hewson to that Discovery against the claims of Dr Monro. It is impossible to doubt, that this would have been much more appropriately placed at the close of the second part of the Inquiries, which contains the

Description of the Lymphatic System in the Human Subject and other Animals. Mr Gulliver, however, retains the original arrangement.

Again, it is well known that the third part of the inquiries consists of five chapters treating successively of each of the following subjects:—1. The description of the red particles of the blood, their physical characters, and the effects of various chemical agents on them; 2. The structure and situation of the lymphatic glands; 3. The structure and uses of the thymus gland; 4. The situation, structure, and uses of the spleen; and 5. The speculations on the formation of the red particles, and the organs concerned in that process, namely, the lymphatics, the thymus gland, and the spleen, that is, the lymphatics and thymus gland in forming the central particle or nucleus, and the spleen in forming the vesicular portion.

Now, when we consider the whole contents of these chapters in connection, it appears to us unquestionable that the proper position for them is the end of the first part of the Inquiries, where they would have completed the whole of what the author had observed as to the history of the blood, and all his speculations on the mode in which its particles were formed. On the merits of these speculations we express no opinion. They may be true or false, well-founded or quite fanciful. That is nothing to the question. But right or wrong, the author connected the lymphatic glands, the thymus gland, and the spleen with the blood and its red particles; and that is quite a sufficient reason for placing this portion of his inquiries in juxtaposition with, and in sequence to, that which relates to the Inquiries on the properties of the blood. Perhaps the second chapter might have been introduced into the Inquiries on the lymphatic system.

We trust that the object and spirit of these observations shall not be mistaken. They are offered, not in any censorious spirit, but solely because we think the arrangement thus suggested would have preserved the unity of the different parts of the treatise, and produced on the minds of logical readers a more agreeable effect than that which results from the present arrangement of the contents of the volume. No one who understands good arrangement, and contrasts it with that which destroys unity, can ever entertain any doubt of the superiority of the former. And we cannot persuade ourselves that, had Hewson's life been prolonged to republish these treatises, he would not have departed from the accidental arrangement of their first appearance, and adopted, if not that now suggested, at least one regulated by the same principles, and founded on the natural alliance of the different subjects treated.

It cannot be necessary to give a formal and detailed account of the contents of writings published so far back as sixty-eight years ago. But these essays contain several points which deserve some present attention, in so far as they form subjects on which, during that long period, much information, collected by different inquirers, has tended to rectify, to confirm, to explain, and to elucidate various inferences formed by Hewson. We shall select only the most important for the subject of present animadversion.

Though Hewson was the first anatomist who examined the blood with any degree of accuracy, and applied to its analysis the penetrative powers of the microscope, yet several curious and important facts had been ascertained previous to his time, and several have been since added.

The fact of coagulation or the separation of the blood into a thin watery liquid, and a solid portion more or less consistent, must have been early noticed and repeatedly observed. The cause of this change has been the great object of inquiry. Harvey was evidently led to speculate on this; but he arrived at no definite conclusions. Willis, who has many curious and fanciful ideas on the blood, was aware of the coagulability of the serum by application of heat and acids. Malpighi obtained the fibrinous part of the clot distinct from the colouring matter, had speculated on the cause of the buffy coat, and discovered the *corpuscula* or globules as they are now described. Lower seems to have ascribed the coagulation to the glutinous character of one of the fluid parts, and in this sense to have made an approach to the idea of the *liquor sanguinis.* The same idea occurred to Borelli, and is stated in more precise terms. Robert Boyle was aware that the serum contains matter coagulable by application of heat, and the addition of spirit of wine or corrosive sublimate.

Samuel Collins had a distinct idea of the difference of the crassamentum and serum, representing the blood to consist of crystalline liquor and red crassament, which coagulates when extravasated, thereby gaining a more solid consistence, produced by manifold fibres. He describes elaborately and correctly the buffy coat, evidently from personal observation, though he was aware of the observations recorded by Malpighi. Bidloo calls the corpuscles *vesiculæ globosæ,* and gives a figure of them. Verduc mentions little globules, and ascribes the coagulation of the blood to their being collected and flattened. Gulielmini examined the blood microscopically, and described the clot as composed of whitish fibres and red globules. The fibrous and globular nature of the blood is maintained by Michelotti, Sydenham, Boerhaave, Haller; and Quesnay.

James Keill who wrote in the year 1708, and may be regarded as the representative of the views of most anatomists at the beginning of the 18th century, distinguishes the blood into two different parts; the red coagulating in a little time, and the serum remaining fluid. "If," says he, "we view a drop of blood with a microscope, we discern a number of red globules swimming in a limpid fluid, and perceive how the globules attracting one another, unite like spheres of quicksilver, which, as they touch, run into one another; and, consequently, the blood divides into two parts."

"After the coagulation of the red globules of the Blood," he continues, "if we examine the serum with a Microscope, we find in it likewise a great number of Corpuscles, of various figures and magnitudes, swimming in a limpid fluid. These do not attract and unite with one another as the former did, till some part of the fluid in which they swim has been evaporated by heat, and then they likewise attract one another, and form a Coagulum, as the globules did."

"This, therefore, is matter of fact, that the blood consists of a simple and limpid fluid, in which swim corpuscles of various figures and magnitudes, and endued with different degrees of attractive force."*

This doctrine he forthwith applies to the formation of a theory of animal secretion, which is founded entirely on mathematical and mechanical principles.

The doctrine now given by James Keill contains one error in supposing that the application of heat to the serum acts by causing evaporation. This it does not; and it is solely by the change which the heat induces in the chemical constitution of the albuminous particles or corpuscles that the coagulation takes place. In other respects, the account given by Keill is correct, and accords very closely with that given by every one who wrote on the subject;—Jurin, Thomas Morgan, Arbuthnot, William Cowper, Martine, Browne Langrish, Northcote, and Berdoe.

Mr Gulliver thinks that the facts above mentioned prove that, previous to the time of James Keill (1702–1708), the spontaneously coagulable matter of the blood was well understood; and that Keill, and those mentioned along with him, and subsequent to him, regarded coagulation as caused simply by the running together of the red corpuscles. But it is to be observed that Keill expressly says that "the blood of all animals, when drawn out of the body, does *naturally* and *of itself* divide into two different parts," the red clot and the serum. This appears to us to be a distinct admission of a spontaneously coagulable part, and a property of spontaneous coagulation. The truth is, that, at this time, physiological writers had no very fixed ideas on the exact cause of the coagulation of the blood; for at one time they seem to have ascribed it to running together of the globules, and at another time to some other cause which made them run together. Such, at least, appears to have been the case with

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* Essays on Several Parts of the Animal Œconomy. By James Keill, M. D. London, 1708. Second Edition. London, 1717.

Boerhaave, Van Swieten, and Haller, the last of whom rather gives the opinions of all his predecessors and cotemporaries than forms any distinct or decided idea of his own. The idea entertained by Keill, of the coagulation being a spontaneous act, and dependent on the mutual attraction of the globules or corpuscles, is evidently more or less adopted from the doctrines of the mechanical and mathematical philosophy, and does not necessarily imply the existence of any vital property in the globules; and when it was once enunciated, it seems to have remained and been repeated in anatomical and physiological writings without much question.

Many speculators followed, not always with very clear and distinct ideas; Francis Home, Sauvages, De Haen, Petit, Quesnay, and Senac. Though the latter appears to have made many observations and some experiments, yet they are confused, and lead to no profitable result, as given in the first edition of his work on the Heart. In his second edition, which appeared in 1774, and which had derived advantage from the experiments of Hewson, several rectifications and a few additions are introduced.

Meanwhile Leeuwenhoek had published in 1688 his microscopical examination of the blood, the bones, the brain, the saliva, and the epidermis; and represented the alleged globules not to be the ultimate elements of the blood, but to consist of many smaller globules, which float in a crystalline, that is, a transparent fluid like water. These ultimate globules, he states, are twenty-five thousand times smaller than a grain of sand; and he adds, that these globules are soft and pliant in the state of health, but during disease become hard and firm,—a fancy which makes him express the opinion, that while considerable hardness of these globules may be the cause of diseases, extreme hardness may induce death.* This globular hypothesis Leeuwenhoek endeavoured to generalize by extending it to the bones, the white matter of the brain, the saliva, and the epidermis; and it is not perfectly certain, that he may not have seen the same objects which have been described by microscopical observers of the present time under the name of nucleated cells.* It must be allowed, that Leeuwenhoek speaks of the globules in Fishes and Birds as flat and oval, and more oblong than broad.

The observations of Leeuwenhoek were first published in the Philosophical Transactions between 1684 and the first ten years of the eighteenth century; but they do not appear to have been very generally adopted till the eighteenth century was well advanced; and then their effect was such, that they caused all the information collected by previous observers to be either neglected or forgotten. It hence resulted, that all the facts regarding the coagulable portion of the blood, the serum, and the formation of the buffy coat, were more or less thrown aside, and little was taught concerning the blood except its minute globular constitution, as delivered by Leewenhoek.

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* Arcana Naturae Detecta ab Antonio Van Leeuwenhoek. Delphis Batavorum, 1695, 4to, T. i. Epistola 65, p. 184.

These notions were, accordingly, more or less generally adopted and repeated by the first Monro, Butt, Gaubius, Davies, and several writers on physiology and pathology. Indeed, it appears, that the microscopical results given by Leewenhoek were substituted almost everywhere for that knowledge of the mechanical constitution of the blood, and its division into coagulable part, red globules, and serum, which had been previously taught. Richard Davies at once, in an essay published in 1759, recalled the original doctrines; but, from some accidental circumstances, this essay never attracted attention.

William Hunter, however, had been directing attention to the same views; and, at this crisis, the experiments of Hewson appeared. Hewson showed very evidently, by numerous and varied experiments, that the coagulation of the blood depended on the presence of the fibrin alone, or that element called since *liquor sanguinis.* Hewson, indeed, was the first who proved the existence of the *liquor sanguinis,* and showed the method of obtaining it.* He did, however, much more. He was the first who clearly proved that the red globules or corpuscles are vesicular and flat, and contain an opake body or *nucleus;* in other words, that the blood-corpuscles are nucleated cells; and in demonstrating this fact, he evidently anticipated the discovery made long afterwards by Schleiden and Schwann as to the nucleated cells of the animal tissues, and the generalization that these are found by microscopical observation in the blood.

"These experiments," says Hewson himself, "not only prove that the particles of the blood are flat and not globular, but likewise, by proving that they are flat, they show that they are not fluid as they are commonly believed to be, but on the contrary are solid; because every fluid swimming in another which is in larger quantity, if it be not soluble in that other fluid, becomes globular."

"It is necessary to remark, that in a few minutes after the particles are spread out on a glass, they run in clusters and stick to each other, and then they appear confused."

This is the account of the vesicle or cell. Then follows that of the *nucleus.*

"When one of these particles is attentively examined, while separate from the rest, it appears, as it lies on its flat side, to have a dark spot on the middle, and round that dark spot it is more transparent. This dark spot was believed to be a perforation, or the particle was supposed to be a hollow ring, by the ingenious

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* Chapter II. Experiments xi. xii. xiii. xiv.

Father Della Torré. But I find, from a great number of experiments, that the dark spot is a solid particle contained in a flat vesicle, whose middle only it fills, and whose edges are hollow, and either empty or filled with a subtile fluid. This is evident to every one who carefully makes the following experiments:"

"1. Take a drop of the blood of an animal that has large particles, as a frog, a fish, or what is still better, of a toad; put this blood on a thin piece of glass, as used in the former experiments, and add to it some water, first one drop, then a second, a third, and so on, gradually increasing the quantity; and in proportion as water is added, the figure of the particle is changed from a flat to a spherical shape. When much water is added, the vesicle by degrees becomes thinner and more transparent, and will at last be dissolved. (It bursts.) When the vesicle has thus assumed a spherical shape, it rolls down the glass stage smoothly, without those phases which it had when turning over while it was flat; and as it rolls in its spherical shape, the solid middle particle (the *nucleus*) can be distinctly seen to fall from side to side, like a pea in a bladder. Sometimes, instead of falling from side to side, the solid middle particle is seen to stick to one part of the vesicle; and in proportion as the vesicle, instead of being flat, assumes a spherical shape, its longest diameter is shortened, as might be expected, on the supposition of its being hollow and flat."

"After this experiment has been made on the blood of such animals as have large vesicles, it may be made on human blood, where the water will be found to have the same effect. The vesicles become spherical; the diameter of these spheres will be less than the largest diameter of the vesicle in its flat state."

"More water is in general required to produce this change on the vesicles of the human blood, than on those of frogs or other amphibious animals (reptiles); and those of the *amphibia* (*reptilia*) require still more than those of fish; for the substance of these vesicles being thicker and more coloured in man and quadrupeds than in the *amphibia*, is therefore later in being dissolved in water; and being thinnest in fish, it thence most readily dissolves. Those desirous of repeating these experiments had best begin with the blood of toads or frogs, whose vesicles are large, and remain some time without dissolving in the water; (used with the above-mentioned precaution); by which means, any one accustomed to microscopical experiments may readily be satisfied of these curious circumstances."

"From the greater thickness of the vesicles in the human subject, and from their being less transparent when made spherical by the addition of water, and likewise from their being much smaller than those of fish or frogs, it is more difficult to get a sight of the middle particles, rolling from side to side in the vesicles, which are become round; but, with a strong light and a deep magnifier, I have distinctly seen it (them) in the human subject as well as in the frog, toad, and skate."

As water renders these particles (corpuscles) round, and makes the dark spot in their middle disappear, Hewson infers, that the red particles of the blood are not perforated; and that the dark spot is owing to something different from a hole,—an inference which, he adds, is confirmed by observing, that though the particle in an obscure light appears only to have a dark spot which resembles a hole, yet, with a transparent lens, and a good light, after diluting the blood with serum, the middle part may be distinctly seen to be only of a deeper red than the rest of the vesicle, and hence appears dark.

Hewson observed also, the ragged appearance, or what he calls the mulberry-like appearance of vesicles in human blood beginning to putrefy, and the rupture of the vesicles in this state, or in their progress to it. He saw in the blood of the eel, the vesicle split and open, and the nucleus escape from its interior exactly as has been witnessed by more recent observers.

In the course of these observations he rectified a very absurd and fanciful notion which was entertained by Leeuwenhoek. That observer thought, that from what he saw by the microscope, each globule was constantly composed of six smaller globules;—a representation which is gravely repeated in Northcote's Anatomy, published in 1772, and which, consequently, may be regarded* as the exposition of the views ordinarily given up to that time. Upon this notion, Father Della Torre appears to have engrafted another, that the vesicles or blood cells have joints, and that, in consequence of this structure, they break uniformly into seven parts. Both of these ideas, which evidently are quite the offspring of fancy, Hewson decidedly controverts.

" The theory of the red particles being composed of six serous ones compacted together, and the serous globules of six of lymph, has not the least foundation, and is entirely overthrown by the simple experiment of mixing the blood with six, or thirty-six times its quantity of water; for the water dissolving the globules ought to reduce them to a yellow *serum*, or colourless lymph; but it does not;—on the contrary, it is coloured red by these particles, even when used in much greater proportion than thirty-six parts to one of blood." He further observes, that from observations he is convinced, that in the number of pieces into which they break, there is nothing regular or constant. He had seen them fall into six, seven, eight, or more fragments, by putrefaction, which acts on them exactly as it does on other animal solids, that is, disintegrates and destroys.

Hewson next refutes another idea repeated by various authorities in those days, particularly Jenty, that the vesicles are oily and

* Arcana Natura Detecta. Epistola 128.

more inflammable than other parts of the blood. That they are not oily is evident from their ready miscibility with water, and their not being acted on by alkalis; neither are they shown by experiment to be more inflammable than the rest of the blood.

Physiologists are fond of discovering uses and the adaptation of means to ends; and in doing so, while they have been anxious to prove their piety, and their veneration for the wisdom of the Supreme Being, they have often evinced the greatest presumption in discovering purposes, objects, and, as a natural consequence, final causes, without ever reflecting that, probably in the whole inquiry, their knowledge is so imperfect, partial, and limited, that they are quite inadequate to the duty which they impose on themselves. Of this a curious example is afforded in the figure of these same blood corpuscles. What, says the philosophic inquirer, during the first half of the eighteenth century, is the reason that they are globular? Manifestly, replies the mechanical physician, to facilitate motion. No body moves in a fluid so easily as a spherical body; no body meets with so little resistance; and no set of bodies could jostle and knock against either with less inconvenience and less injury. Hence the great advantages of the globular figure. The fact is nevertheless incontestible, that these bodies are not globular, but flattened disks, usually with a depression on each surface. When Hewson discovered that these corpuscles are positively flat and diskoidal, then it was argued, that they might be globular within the body and during life, but become flat when removed from the body. In answer to this, Hewson maintains, that he is convinced they are as flat within the body as out of it; and that, by repeatedly observing them while circulating in the small vessels, between the toes of a frog, both in the solar microscope and in the simple one with an additional lens, he satisfied himself that they were flattened disks; and he saw them with their sides parallel, like a number of coins laid one against another.

Not only are these bodies not globular, but it appears, adds Hewson, that by a peculiar provision they are made to retain the diskoidal figure, and they never assume the globular figure unless under unnatural circumstances, and when they are advancing to disintegration. This provision consists in the saline character of the *serum*. The addition of any of the neutral salts also, as Glauber's salt, Epsom salt, nitre, nitrate of ammonia, nitrate of magnesia, and muriate of soda, caused the vesicles to shrink and become shrivelled round the *nuclei*, so that they appear to be solid and strongly drawn round the *nucleus*. As to acids, he found that acetic acid, as has been since constantly done, and the sulphuric, nitric, and hydrochloric acid, when much diluted, dissolve the vesicles, without making them spherical.

Hewson accordingly infers that the principal use of the salts of the blood or serum is to preserve this diskoidal shape in the globules. He gives, however, no opinion as to the object or purpose of this flattened or diskoidal shape; and it seems difficult to do so. The only probable idea that at present occurs, as of the smallest use in explaining the fact of the flat diskoidal figure, is this,—that it is now ascertained that most of the textures of the animal body, the skin, the mucous membranes, the serous membranes, muscular fibre, bone, the final elements of the glands, the membranes which form the blood-vessels, are formed by the regular and symmetrical connection of nucleated cells which are flat, circular, or hexagonal; and as these textures must be originally formed from the blood, and afterwards maintained and nourished by that liquid, this shape may be given to the blood-cells either on the principal of unity, so generally observed to take place in animal and vegetable formations, or it may be given them in the transition to the cell figure of the nucleated cells of the several tissues which they are destined to nourish. This idea, nevertheless, is submitted with the diffidence and hesitation with which all such inferences ought to be deduced.

Hewson found the same flat vesicular appearance in the blood of the *articulata*, as the lobster and the shrimp; and in the *monoculus* of the river water of London. Of *nuclei*, however, in the blood of these animals, he says nothing.

A short summary of these views Hewson communicated to Dr Haygarth in a letter to that physician of the 19th July 1773, and which was afterwards published in the Medical and Philosophical Commentaries of Edinburgh.*

It would be very desirable to compare the description of the nucleated cell of the blood as given by Hewson, with that given by Henle,† which is on the whole the most accurate and the most complete, previous to that given by Mr Wharton Jones. But this would extend too much this article, already sufficiently long; and we therefore defer the account given by Henle to another season. Of the leading facts ascertained by Mr Wharton Jones, a detailed account is given in the sixty-seventh volume of this Journal;‡ and to that we at present refer our readers.

Some of the points here adduced require modification. The demonstration of the nucleus and its cell-wall in the corpuscle of the lower vertebrated animals, that is, the frog, toad, and skate, is quite correct; and its accuracy has been confirmed by subsequent observations. But it is now generally allowed. that a mistake was committed in stating that a similar nucleus exists in the blood-corpuscle of the human subject. It seems to be ascertained, from the researches of Mr Gulliver and Mr Wharton Jones, that the nucleus, if it do exist, is formed only in the embryonic period of the life of the mammiferous family of animals. In the adult state of the mammiferous family, and in man, the majority of the red corpuscles present no nucleus corresponding to that, so distinctly, and, in general, easily, recognized in the lower VERTEBRATA. It is, indeed, only in the oviparous VERTEBRATA, that nuclei can be recognized in the corpuscles.

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* Vol. iii. p. 87. London, 1775.

† Allgemeine Anatomie, u. s. w. Seite 427. Leipzig, 1841.

‡ Edinburgh Medical and Surgical Journal, vol. lxvii. p. 529.

What appears to have led to this mistake is the circumstance, that granular spots, either circular or variable in form and size, often occur in the blood disks of a mammiferous animal, in consequence of puckering of the envelope or cell-wall, shrinking in one part and swelling in another, from a variation in the contents of the corpuscle. Similar appearances, which imitate the aspect of the *nucleus*, may also be observed on adding muriatic acid or saline solutions to the blood under the microscope.

Mr Gulliver, in short, is of opinion that the nucleus exists in the embryonic state in mammiferous animals, but disappears as the fœtus approaches in growth the full time. In the very young mammiferous embryo, the corpuscles are much larger than in the adult. In these two circumstances, the physiologist recognizes one of those points of unity in formation, by which the embryo period of the mammiferous family is brought near to the adult formation of the oviparous VERTEBRATA.

An inquiry, to which Hewson evidently attached great importance, was that of the mode in which these red particles or blood-cells are formed, the agency by which they are formed, and the organs in which these formations are effected. It was a happy conjecture ascribed to him by Mr Falconar,—that though "the figure and size of the particles of the blood differ in different animals, yet the general conformation of vesicle and particle (cell and nucleus) extends probably throughout animal nature."[*] It is now known, that the arrangement here referred to extends not only through the animal kingdom, but over the still more extensive domains of the vegetable world. The cytoblast is a formation of universal existence. Whether it was, that this idea of the possible universality of the nucleated cell had made him look for the formation of these cells in the lymphatic system, or, conversely, whether the immense space which, in his eyes, the lymphatic system occupied among organic textures led him to consider it as the great manufactory,—the incessantly acting workshop of nucleated cells,—certain it is, that in this system, which his surviving relative takes care to define as the Lymphatic vessels, the Lymphatic Glands, the Thymus, and the Spleen, he fixed the preparation, construction, and completion of these bodies.

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[*] Experimental Inquiries, part ii. chap. v. section 80. London, 1777.

To establish this hypothesis, we are informed,—first, that the lymphatic glands secrete a fluid which, when examined by the microscope, presents numberless small solid particles (*nuclei*), exactly resembling in size and shape the central particles contained in the vesicles of the blood. The lymphatic vessels issuing from the cells of the lymphatic glands be denominated the excretory ducts of these glands, from which they convey the secreted particles into the lymphatic vessels passing through the gland, and thence into the thoracic duct and blood-vessels.

Secondly, we are told that, if we open a lymphatic vessel which has emerged from a lymphatic gland, we find not only a great number of these central particles, but also many of the particles of the blood completely formed,—that is, the central particle is surrounded by a vesicle; in short, it is a complete nucleated cell. From this circumstance, he thinks that it may be conjectured, with a great degree of probability, that the vesicle is either a secretion from the internal coat of the lymphatic vessel, or that the lymphatic vessel has a plastic power over its contained fluid, so as not only to form a vesicle round the central particle, but also to give it its red colour; for, till the red vesicle, he adds, is formed, the central particle is evidently white.

The same power of forming *nuclei*, or central particles, Hewson likewise ascribed to the Thymus gland, which he regarded as merely a large lymphatic gland, accommodated to the wants and necessities of the fœtal condition.

As, however, many of these central particles appear to be moving in the lymphatics and the thoracic duct without vesicles or cell-walls, Hewson had recourse to the spleen as an organ to supply this deficiency. That the spleen, we are assured, really does secrete the vesicular portion of the red particles of the blood, we have very convincing proofs; 1st, in the circumstance now stated; 2d, in the position that the spleen does secrete something; 3d, that blood from the splenic vein contains no central particles; and similar considerations; all of them certainly as arguments rather questionable.

In all this hypothesis, it seems never to have occurred either to Hewson or Falconar, that if one animal fluid contains cells or vesicles with central nuclei, another animal fluid may do the same. It has been since ascertained, that most of these fluids contain either cells, granule cells, or nucleated cells.

Mr Wharton Jones has criticised this part of Hewson's representations, that the central particles are at first free, and that they acquire subsequently the surrounding vesicular portion or cell-walls. This mistake, he thinks, is to be explained by the fact, that, in the fluid of the lymphatic glands in birds, nucleated cells in the uncoloured stage are very numerous; and that while the nucleus is very distinct, the cell-wall is extremely pale. From this circumstance, and probably, we add, from the microscope which he employed being less powerful than those now in common use, he naturally inferred that these *nuclei* were void of cell-walls, and that, when the cell-wall became evident by its acquiring colour, it was a new formation.

It is not so easy to explain his inference as applied to the lymph of man and the Mammifera. The red corpuscle of the blood of the Mammifera has no central particle; and the description of Hewson, of the central particles of the fluid of the lymphatic glands of the Mammifera, cannot be applied to the cellaeform *nuclei* of the uncoloured nucleated cells, because the cellaeform nucleus of the uncoloured nucleated cells is nearly, if not quite as large as the perfect red corpuscle itself. For some other observations on this point by the same author, we refer to his paper in the Philosophical Transactions,* and the summary of it given in this Journal.†

The whole of the preceding hypothesis, indeed, regarding the mode in which blood-cells are formed, is still in a condition of great uncertainty. The structure of the thymus gland has been since that time studied carefully by Sir Astley Cooper and Mr Simon; yet their researches have elicited nothing, which can be said to give any support to the opinion, that it assists in the formation of blood-globules. Bischoff alone partially supports the view of Hewson. The spleen has been also subjected to many examinations by Sir Everard Home, Dr Hodgkin, Tiedemann and Gmelin, Schultze and Donné, and several others, since the time of these inquirers; yet are we as remote as ever from having ascertained anything regarding its share in the fabrication either of vesicles or corpuscles. Is it to be inferred from the mode in which hypertrophy of the spleen in certain cases causes death, that it has any more immediate or direct influence on the blood in the veins, than other vascular organs when in a state of disease? In certain forms of hypertrophy, it has been ascertained that death takes place in connection with, and probably in consequence of purulent matter in the veins of the brain, those of the lungs, and other organs.‡ The question suggested by such cases is, how is the purulent matter formed; and why should it be formed in enlargement of the spleen and not in that of other organs? Is there really anything peculiar in the blood of that organ, that should cause it, under certain circumstances, to give rise to the presence or formation of purulent matter in the veins? On all these points we require a greater number and variety of facts.

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* Philosophical Transactions for 1846, part II. London, 1846.

† Edin. Med. and Surg. Journal, vol. lxvii. p. 547. Edinburgh, 1847.

‡ Two cases of disease and enlargement of the spleen, terminating in death, from the presence of purulent matter in the blood.

Case of disease and enlargement of the spleen, in which death took place from the presence of purulent matter in the blood, by D. Craigie, M.D., &c.

Case of hypertrophy of the spleen and liver, &c., by J. H. Bennett, M. D. Edinburgh Medical and Surgical Journal, vol. lxiv., p. 400. Edinburgh, 1845.

A discovery, which shows the acuteness of Hewson as much as any other fact, is that of the cause of white or milk-like serum, which, by means of the microscope alone, he traced satisfactorily to the presence of oil-globules. An abstract of Hewson's facts and the inferences deduced from them, along with an example of this disease, is given in the thirty-third volume of this Journal, in such detail as to render it unnecessary again to enter on the subject.* In a subsequent part of the same volume is given, by Dr Christison, an example of the change, with a very neat and effectual mode of proving the presence of the oil by the addition of sulphuric ether. Dr Christison showed that by agitating the milky serum with ether, the oily matter is detached by the latter and made to float on the surface of the serum, which is then clear, as serum usually is. Very nearly at the same time, namely, in 1830, Dr Babington, of London, communicated to the Medico-Chirurgical Society an ingenious paper on the milkiness of the serum, in which he had employed the same agency to separate and show the oily matter.† The history of this morbid condition of the blood is therefore now so far complete, in relation to the fact and the cause of the milkiness; and the best mode of demonstrating the fact, that the presence of oil is the cause of that milkiness. In some other points the history is imperfect. Hewson seems to think that oil exists in the blood only in certain states of deranged health; and assuredly this has been an opinion pretty generally entertained since the time of Hewson. Dr Babington, on the other hand, obtained oil from healthy serum, and concludes, therefore, that it is constantly present in the most healthy serum; and that it is only when it is in such abundance as to render the serum opalescent or milky, that it can be regarded as amounting to disease. Oil, in short, according to Dr Babington, exists in the blood as a general constituent of that liquid. Its quantity seems, however, to have considerable influence on the specific gravity of the serum. The density of that liquid in the healthy state may be stated, he says, at 1029. When, however, the serum is milky, that is, when it contains an abnormal proportion of oil, the specific gravity falls so as to range between 1024 and 1019. Further, as the specific gravity of serum bears a direct ratio to the quantity of albumen, Dr Babington thinks it likely that, when oil is superabundant, it is so at the expense of the albumen.

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* Edinburgh Medical and Surgical Journal, vol. xxxiii. p. 215 and p. 274.

† An account of a concrete oil existing as a constituent principle of healthy blood, in a letter addressed to the Secretary of the Medico-Chirurgical Society, by Benjamin G. Babington, M.D., F.R.S. Medico-Chirurgical Transactions, read February 28, 1830. Vol. xvi. p. 46. London, 1830.

All this, however, throws but little light on the causes which increase to so great a degree this oily matter, if it be regarded as a necessary constituent of blood. It is to be observed that the morbid states of the system in which milky or oily serum has been witnessed, are chiefly those in which the action of the digestive and assimilative organs is more or less suddenly impaired, and the individual, after being a free liver and rather large eater, is emaciated; and in granular or fatty degeneration of the kidney. Is it to be inferred, therefore, that the oily matter which no longer is deposited in the adipose tissue is floating about in the blood, and thus undeposited, is the cause both of the symptoms under which the patient labours, and of the oily serum? This fact appears now to be established by the observations of Mr Gulliver, Dr Johnson, Mr Toynbee, Gluge, and various other observers, that, while fat or oil, when deposited in the adipose tissue, produces no inconvenience to the economy, if deposited in other organs and textures concerned in the great functions necessary to life, it produces the most serious evils, and is associated with lesions apparently incurable. Thus fat deposited in the arteries renders them brittle and unyielding, and unfit for their functions as elastic transmissile tubes. Fat deposited in the glandular elements and tubules of the kidney induces granular or fatty degeneration of that gland,—a lesion almost certainly fatal. Fat deposited in the *akini* of the liver causes in that organ changes incompatible with the exercise of its secreting functions, and scarcely less certainly fatal. Fat is deposited in the tissue of the lungs in hepatization, and in tuberculation of these organs, and forms a characteristic element of these morbid changes. Fat infiltrated into the muscular tissue of the heart is attended with softness and lacerability of that organ, enfeebles its action, and is an occasional cause of sudden death. And, lastly, fat constitutes a considerable proportion of the new deposit in *Enkephaloma*. In all these instances, the presence of fat or oil in situations where it ought not to be, not only impedes the performance of the functions of the organ, in which it is deposited or infiltrated, but is connected with various severe and unmanageable forms of deranged health, which almost invariably terminate in the death of the individual.

The fact that in all these instances of extra normal or misplaced deposition of fat, the patients lose flesh, and the most corpulent persons become wasted and meagre, with great debility, seems to show, that the idea of Dr Babington that the oil is deposited at the expense of albumen, is, in some degree, well founded. Liebig and Gregory, in their recent edition of Turner's Chemistry, allow that albumen may, in certain circumstances, be converted into, or furnish the elements of fat, and that fat may be derived from albuminous matters when soda is deficient. All the elements of food, indeed, contain a larger proportion of oxygen than fat does, which possesses only 10 equivalents of oxygen for 120 of carbon, or about one-twelfth part. In order, therefore, to convert albuminous matters in the living body into fat, either the carbon must be increased, or the oxygen must be diminished. The individual or individuals consume, it is understood, the same proportions of carbonaceous matter, oxygen, nitrogen, and hydrogen, in the different articles of food and drink; yet there appear in the solids and fluids considerably more carbon, much less oxygen, and probably less nitrogen. The question then presents itself, by what means is the carbonaceous matter increased and the oxygen diminished, so as to present the superabundance of oil in the blood? It must not be forgotten, in applying this chemical reasoning, that the whole tissues of the patient, at least the albuminous and fatty tissues, undergo a most serious process of detrition and wasting; so that it appears, as if the assimilative powers, which in health formed the albuminous parts in due quantity, and the oleaginous not in excessive proportion, were so enfeebled that they are now unable to effect this purpose.

Another question is suggested in this view of the source of the oily superabundance. This is, what becomes of the nitrogen, which must be let loose in this transformation of albumen into fat. Is it employed to increase other nitrogenous elements, and what are they which are thus increased? This appears to present to the hypothesis of the transformation of albumen into fat a difficulty which has yet to be explained.

Another view of this phenomenon remains yet to be taken. It is ascertained that the chief source of fat in the animal body is starch or sugar, the composition of which is such that, when deprived of oxygen, fat remains. Thus starch is represented by $C^{12} H^{10} O^{10}$; and if from this we deduct 9 atoms of oxygen, the residue is $C^{12} H^{10} O$, which is one of the empirical formulae for fat. Or again, if from the formula for starch, we deduct one atom of carbonic acid, that is, CO^2, and seven atoms of oxygen, the remainder is $C^{11} H^{10} O$, which represents the other empirical formula for fat. In either case, the convertibility of starch, and consequently all amylaceous articles of food into fat, becomes manifest.

Starch, in different phases, forms a considerable proportion of the alimentary articles; and, if we suppose that the alimentary organs are no longer able to transform these articles in their due proportions, along with the nitrogen of the albuminous articles into the textures, it is conceivable, that by some deficiency of oxygen, caused probably by imperfect or inadequate respiration, the oil may be furnished in undue quantity.

At the same time, we own, that in these chemical theories we have at present little confidence. What may take place within the animal body in certain circumstances, is here shown. What does actually take place, and the manner or mechanism by which it is effected, remains yet to be revealed.

After all, we doubt whether the old theory propounded by Hewon, and repeated by Gregory, senior, and other speculators, that the fat is simply resorbed or taken up and conveyed into the blood, be not the most probable. We understand, nevertheless, that the important fact established by the researches of modern pathologists, as to the pernicious and eventually fatal effects of fat and oil being infiltrated and deposited in situations where these substances ought not to be, remains with all its original value undiminished.

We have left so little space to notice the second part of the Experimental Inquiries on the Lymphatic System, that we must rapidly hasten to a conclusion. The most interesting part of the Description of the Lymphatic System is certainly the twelfth chapter, in which the author treats of the intestinal *villi*, and their relation to the lacteals.

These piles, or *villi*, though seen by many anatomists, were first examined in 1721 by Helvetius, who represents them as cylindrical prominences in quadrupeds, but conical in the human subject.* Their intimate structure, however, Lieberkuhn undertook first by microscopical observation to demonstrate. According to this observer, each *villus* receives a minute branch of a lacteal, arterial branches, a vein, and a nerve; and in each the lacteal branch is expanded into a minute sac or bladder (*ampullula, vesicula*), like an egg, the capacity of which he estimates at ⅛th of a cubic line, and in the apex of which may be seen by the microscope a minute opening.† Upon this sac the arterial branches are ramified to great delicacy, and terminate in minute veins, which then unite into one trunk; while its inner surface he represents as spongy and cellular. The space between the *villi*, which do not touch each other, he further represents to be occupied by the open orifices of follicles, so numerous that he counted eighty of them, where were eighteen villi; and both, he asserts, are covered by a thin but tenacious membrane similar to epidermis.

Hewson, while he adopted the idea of Lieberkuhn, that the lacteals open within or on these *villi*, proves that this was the mode of communication, at least in birds, fishes, and reptiles. He denied, however, the existence of the small *sacculus or ampullula*, into which, Lieberkuhn maintained, each *villus* was expanded; while he conceived he had proved in the *villi* the existence of pores or imbibing orifices, and a property of erection or distension when they were absorbing the chyle from the interior of the intestines. This communication of the lacteals with the *villi*, some half-informed persons have lately attempted to ascribe to Henle and other recent observers. The truth is, however, that Hewson had demonstrated the connection by means of injections, as is still seen in some of his preparations preserved in the Museum of the College of Surgeons, London.

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* Mem. de l'Acad. des Sciences. 1721.

† J. N. Lieberkuhn, M. D. &c. Dissertatio Anatomio-Physiologica de Fabrica et Actione villorum Intestinorum Tenuium Hominis. Lugduni Batavorum, 1745. 4to; et cura J. Sheldon. Londini, 1782, § ii. iii. &c.

Hewson further admits in each villus the ramification of minute arteries and veins, and infers that the lacteals are ramified in the same manner as the blood-vessels, and that the whole constitute a broad flat body,* the spongy appearance of which he ascribes to the mutual ramification of the latter.

In denying the existence of the *ampullula* or dilated bag-like cavity in each *villus*, Hewson was led, by what he observed, to admit no more, than what could be demonstrated. He argued that Lieberkuhn was deceived by appearances; and that what he mistook for the *ampullula*, was in truth a cluster of vessels filled to distension by injection. The same appearance, he argued, was presented by the *corpora globosa* or Malpighian bodies of the kidney, which, after the example of Ruysch, he contended, were not *sacculi* or follicles, but clusters of blood-vessels. This is the most unfortunate and feeblest argument that Hewson has adduced; and it shows, probably better than any other circumstance, that his optical instruments were not so powerful as they ought to have been. In one sense, indeed, Hewson was right as to the existence of vessels; but he was mistaken as to the non-existence of the spherical or spheroidal bladder or vesicle. It is now ascertained that the *corpora globosa*, or Malpighian bodies, consist of small spherical or spheroidal vesicles, with a most copious and minutely ramified system of cirrhose or curling arteries all round them, penetrating their membrane, and conveying blood which is brought back by another system of vessels. But as we fully explained the peculiarities of this structure from the researches of Muller, Mr Bowman, Huschke, Reichert, Gerlach, Bidder, and Kolliker, in our sixty-eighth volume,† to that we refer for details. The truth is, that the minute structure of the glands had not at that time been so thoroughly and carefully investigated as it has since been; and consequently was less fully and clearly unfolded. On the communication between the *villi* and the lacteal vessels, anatomical opinion has been, if possible, still more discordant.

Cruikshank maintains that the *ampullula* is altogether imaginary—a piece of invention on the part of Lieberkuhn; and he is certain, from careful observation, that the spongy cavity, of which Lieberkuhn speaks as an ampullula, is the common cellular membrane of the villus, connecting all the arteries, veins, nerves, and lacteals together. Cruikshank had further seen, in the body of a female suddenly cut off by convulsions, the villi distended, as well as the lacteals, with chyle; and the *villi* then resembled white vesicles. By attentive examination with the microscope he ascertained the following facts. 1. Many of the *villi* were so filled with chyle, that he saw no ramifications of arteries or veins; and the whole, we suppose, of each *villus* appeared like one white vesicle without red lines, pores, or any orifice. 2. Others of the villi contained chyle only in small proportions; and in these the veins were numerous, and by their red colour predominated over the white colour of the *villi*. 3. In some hundred *villi*, he saw the trunk of a lacteal rising by radiated branches. The orifices of these radiating vessels were very distinct on the surface of the *villus*, as well as the *radii* themselves, seen through the external surface, passing to the trunk of the lacteal. They were full of white fluid. He observed only one lacteal trunk in each villus.*

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* Experimental Inquiries, part ii. p. 175, chapter xii.

† Edinburgh Med. and Surg. Journal, Vol. lxviii. p. 449. Edin. 1847.

Sheldon, on the other hand, who, in 1782, published at London an edition of the Dissertation of Lieberkuhn, found the *villi* not only round and cylindrical like Hewson, but bulbous as Lieberkuhn, and sabre-shaped, and is disposed to maintain the existence of the *ampullula* or dilated cavity.†

Mascagni and Soemmering, agreeing in the general fact of a union of vascular and lacteal structure, seem to represent the shape of the *villus* as that of a mushroom, consisting of a stalk and a *pileus*.

Some of these discordant statements Hedwig attempts with equal ingenuity and industry to reconcile. The difference in shape he refers to differences in the animals examined; and in one class finds them cylindrical, (*e. g.* in man and the horse); in another conical, (the dog); in a third club-shaped, (the pheasant); and in a fourth pointed or pyramidal, (*e. g.* the mouse). The interior structure he also represents as spongy in all the animals which he examined; and invariably also he found at the apex the orifice of the duct, which, after the example of Lieberkuhn, he conceives constitutes the *ampullula*.‡

These conclusions are not exactly confirmed by the researches of Rudolphi, who examined the *villi* in man and a considerable number of animals. This anatomist never found the orifice seen by Hedwig, notwithstanding every care taken to perceive it; and for banishing this orifice from anatomy, he is greatly commended by Henle. He maintains that the *villi* are not alike in all parts of the intestinal canal of the same animal, as represented by Hedwig, but may be cylindrical in one part, club-shaped in another, and acuminated in third. Admitting their vascular structure, which he thinks may be demonstrated, he regards the ampullular expansion as doubtful, and denies its cellular arrangement.*

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* The Anatomy of the Absorbing Vessels, by William Cruikshank. Second edition. London, 1790. Chapter xii. pp. 58 and 59.

† The History of the Absorbent System, pp. 36 and 37.

‡ Disquisitio Ampullularum Lieberkuhnii Physico-Microscopica. Lipsiæ, 1797. 4to.

About the same time Bleuland, who had previously examined the intestinal mucous membrane, after successful injection of its capillaries, undertook to revive the leading circumstances of the description of Lieberkuhn. By examining microscopically well-injected portions of intestine, he shows that the *villi* are composed of a system of very minute arterial and venous capillaries, enclosing a lacteal which constitutes the *ampulla*, and in the interior of which a certain order of these capillaries terminates. He also revives the statement of the absorbing orifice at the extremity of each *villus*.†

According to Beclard, the intestinal *villi* appear neither conical nor cylindrical, nor tubular nor expanded at top, as described by several authors, but in the shape of leaflets or minute plates so closely set that they form an abundant tufted pile. Their shape varies according to the manner in which they are examined, and according to the part. Those of the pyloric half of the stomach and *duodenum* are broader than long, and form minute plates; those of the *jejunum* are long and narrow, constituting piles; at the end of the ileum they become laminar; and in the colon are scarcely prominent. They are semitranslucent; their surface is smooth; and neither openings at their surface, or cavity, or their interior, or vascular structure can be recognized.‡

Since the time of Beclard, the connection between the *villi* and the lacteals has been examined by Krause, Henle, Schwann, Vogel, Wagner, and Böhm. Yet the accounts given are not much less discordant.

The *villi*, according to Henle, are, when empty, flat, partly hair-shaped, long and small, and with an attenuated basis, partly valve-shaped, with broad basis, and arched free edge. When the lymph-vessels are filled, the small villi are cylindrical. The length of these varies from 0.25 to 0.33 of one line; the diameter of the cylindrical ones from 0.07 to 0.08 of one line. They are formed by the mucous membrane of the intestine, which, covered by its slender epithelium, projects into the cavity of the intestine, like the finger of a glove, or like a small fold. The small *villi* have a simple central cavity, which, closed at the apex, occasionally begins expanded something in the gourd shape, and is continued along the axis to the base. The broad *villi* have, in like manner, either one simple canal, which commences closed on one side, and is continued along the arched edge, to be lost in the substance of the opposite edge; or they have two canals, which arise together from the highest part of the fold or *plica*, with shut, often branch-like incurvated apices, and thence diverging, proceed onward to the lateral margin of the plate. By examining microscopically the *villi* stripped of epithelium, these canals are seen bounded by two dark edges. They are seen on transverse sections of the *villi* as round openings; and in villi filled with chyle they are of a silvery white colour.

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* Einige Beobachtungen über die Darmzotten von D. Karl A. Rudolphi, in Reil. Archiv. iv. b. 1797, pp. 63 and 340. Und Anatomisch-Physiologische Abhandlungen von Karl Asmund Rudolphi, Mit Acht Kupfertafeln. Berlin, 1802. 8vo. iii.

† Jani Bleuland, M. D. &c. Vasculorum, in Intestinorum Tenuium Tunicis, &c. Descriptio Iconibus Illustrata. Trajecti ad Rhenum, 1797.

‡ Anatomie Generale, chap. iii. sect. 2de, p. 253.

When we trace the lymphatics which accompany the blood-vessels of the intestine, and which, when they contain chyle, are easily found between the meshes of the net-work, towards the cavity of the bowel, we observe them forming, in the interstitial layers between the individual tunics of the intestine, nets, which proceed to the external surface of the mucous membrane. Two layers may be distinguished,—an inner between the mucous and muscular coat, and an outer between the muscular and serous coat. From both net-works proceed fine branches in an oblique direction to the lymphatic glands, on the concave edge of the intestine. From the smallest ramifications of the internal net-work proceed, without becoming remarkably smaller, twigs at a right angle towards the cavity of the bowel, and into the *villi;* and these are the twigs which form the above described central canal of the *villi.* When the canal is examined from the inner surface, we observe a minute shoot, which, being covered by the inner layer of the mucous membrane, and having consequently a less vivid brilliancy, runs horizontally, gives off branches right and left, which ascend to the *villi*, and which at length terminates in a *villus.*

Some years previously, Henle saw the beginning of the lacteals in the *villi* of a man who died during digestion, and in which they were strongly distended by chyle. In the same specimen, Schwann demonstrated the middle canal by injection with quicksilver from the lymphatics of the mucous membrane. The same fact Vogel and Wagner observed in similar instances. In slight turgescence, the central canal is often indicated by a continuous row of large fat globules. Very frequently in man and animals, only the apex of the canal is filled with fat-drops, which may be dispersed by pressure, and moved onwards through the canal to the root or base of the *villus.*

It is nevertheless not quite certain, according to the observation of Krause, whether this canal is the actual commencement of the lacteal vessel. According to Krause, the lymphatic or absorbing twig arises in the middle of the *villus*, the width of which does not exceed 0.0139 of one line, from several small absorbent vessels, which arise partly with free ends, partly communicate in the meshes of network. The largest of these absorbents, which pass immediately into the principal vessels, have a diameter of 0.0123 of one line; the smallest of 0.0061 parts of one line.

As to the *ampullula*, or dilated saccular cavity, that has found in Böhm a sort of defender. This author saw in cholera patients, what is seen occasionally in the bodies of other persons, that the *villi* contained at their free apices a drop of fat. From the cavity in which the drop was contained, it could be urged, as already stated, within the central canal to the base of the *villus*. Whether this took place through a normal aperture Böhm leaves undecided; and, in the opinion of Henle, this experiment may be explained without admitting such an aperture. The cavity in which the fat-drop is found, is, Henle thinks, evidently the beginning of the lacteal; and if any one choose to name this, from the gourd-like expansion which it occasionally shows, *ampullula*, there is nothing to object. But Lieberkuhn, he adds, held for the ampullula a peculiarly formed cavity, in which the vessels open, and which, by cellular partitions, is divided into compartments.

It thus appears that the whole dispute is a controversy about words. There is one central canal or two, which form the beginning of the lacteal. This canal one or two observers call an *ampullula*. Others call it the beginning of the lacteal, and the central canal of the villus.

The doctrine that the lymphatic vessels form an exclusive system of absorbents, has now lost much of its interest, since the possession of this property has been restored to the veins, which certainly perform this function as well as the lymphatics. The lymphatics, indeed, must be viewed as a sort of appendage and auxiliary system to the veins.

It was early shown by Dr Foart Simmons, that William Hunter was mistaken when he believed that he was the first to ascribe this property to the lymphatics. Mr Henry Watson showed this gentleman a passage in the anatomical treatise of M. Noguez, published first in 1723 and afterwards in 1726, which clearly proves that M. Noguez considered the lymphatics as absorbing vessels.* To Hunter, nevertheless, and his pupils, belongs the merit of examining these vessels so carefully, and giving so complete and accurate an account of their history and functions, that subsequent anatomists have had little more to do than confirm and verify their statements.

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* "Les seconds vaisseaux lymphatiques sont veineux; ils rapport la lymphe dans les vaisseaux sanguins ou dans les veines; ils y en a dans toutes les parties du corps; ils repompent la matiere lymphatique qui s'evacue par les premiers. Ou peut les nommer conduits absorbants." L'Anatomie du Corps de l'Homme en abregé, par M. Noguez, Medecin du Roy, &c Paris, 1726. 8vo. Apud, an Account of the Life and Writings of the late William Hunter, M. D., &c., by Samuel Foart Simmons, M. D., &c. London, 1783.

It is time, however, that we close these observations. We began with William Hunter, because it seems impossible to speak of any of his pupils or their labours without thinking of that remarkable man; and with William Hunter we must conclude.

About the year 1773, when, in the fifty-fifth year of his age, Dr Hunter began to suffer much from symptoms of wandering gout, notwithstanding an habitually abstemious mode of living. In the course of a few years, the fits became more frequent, affecting sometimes the limbs, and sometimes the stomach. At length, on Saturday, the 15th March 1783, he suffered so much from head-ach and squeamishness, that he was compelled to remain in bed. On the following Thursday, 20th, he had so far recovered, that he resolved to deliver the introductory lecture to the operations of surgery. He did so; but fainted away at the close of the exertion. He was conveyed to bed; and departed this life on Sunday, the 30th March 1783, aged not quite sixty-five years.

The school which he founded, and which was in his lifetime established in Great Windmill Street, long continued to prosper as a great and useful institution, under a succession of teachers little less distinguished than the founder. Of John Hunter, Hewson, Cruikshank, and Sheldon, we have already spoken. These were followed by Wilson, Matthew Baillie, Brodie, and Sir Charles Bell. The Windmill Street School no longer exists. But its remembrance must be preserved from oblivion by the names of the men, who for more than fifty years continued there to instruct numbers of zealous disciples.

It is useful as well as interesting to trace events to their primary and their accessory causes. William Hunter, a young man without friends, succeeded in effecting a complete and useful revolution in the mode of teaching anatomy in London. Besides enabling Hewson and Cruikshank to publish their treatises on the lymphatic system, he was the cause of his brother, John Hunter, being the highest physiological authority and the first surgeon in the metropolis, and leaving to the College of Surgeons the most extensive and valuable assemblage of facts in anatomical and physiological science ever collected by the labour of one individual. Their sister, Dorothea Hunter, became the wife of the Reverend James Baillie, D. D., afterwards Professor of Divinity in the University of Glasgow, and the mother of Matthew Baillie, long afterwards the first physician in London, and of Joanna Baillie, the authoress of dramas which make a near approach to the grandeur and sublimity of the ancient Greek tragedy. No one could have predicted, one hundred and seven years ago, that one event was to produce so many and so great results.

ART. II.—*Observations on the History and Treatment of Dysentery and its Combinations; with an Examination of their claims to a Contagious Character, and an Inquiry into the Source of Contagion in Analogous Diseases;* ANGINA, ERYSIPELAS, HOSPITAL GANGRENE, *and* PUERPERAL FEVER. By WILLIAM HARTY, M. D., Physician to the King's Hospital and the Prisons of Dublin, &c. &c. Second edition. Dublin, London, Edinburgh, 1847. 8vo, pp. 297.

THE first edition of this work appeared in the year 1805, and presented a sketch of the nosological history of dysentery and its complications, with an inquiry into the question of its contagious or non-contagious character; which must be allowed to be a useful and instructive contribution to the medical literature of that period. The author at that time, twenty-five years of age, acknowledges that he wrote the treatise without much personal experience in the nature of the malady, which formed the subject of consideration and inquiry. But he studied attentively what had been recorded by previous authors, who had witnessed the distemper in various climates, in different countries, and at different periods; and he subjected the evidence thus adduced to a scrutiny which evinced learning, reflection, and judgment.

The chief object of the author at that time was, to inquire what evidence the several epidemics of dysentery recorded by authors furnished, to show, whether the disease was contagious in its mode of propagation, or was propagated without any contagious property; and he arrived at the conclusion, that, though dysentery, when either idiopathic or associated or complicated with intermittent or remittent fever, is void of contagious character, yet, when associated with typhous fever, it is propagated after the manner of a contagious disease. The author believed further, that, in drawing the conclusion now stated, there was sufficient evidence to show that the distemper is, in certain circumstances, associated with, or complicated or accompanied by, typhous fever. Dr Harty, in short, arrived at the conclusion, that, whereas it had been fully established by the testimony of multiplied observers at home and abroad, in civil and in military and naval practice, that dysentery often springs up in the same situation and under the same circumstances as endemic fever,—ague, or remittent,—and is a telluric distemper, in such circumstances, it is quite free from contagion; yet that, under certain circumstances favourable to the origin and propagation of typhous fever, it is also liable to take place, and then it is attended with the same contagious properties, by which the progress of that disorder is distinguished.

In furnishing further illustration and confirmation of this view, Dr Harty considered shortly the circumstances under which certain other diseases arise and are propagated,—for instance, catarrh, *angina maligna*, ophthalmia, erysipelas, hospital gangrene, and puerperal fever,—from the history of all which diseases the author derived inferences more or less strong and conclusive, in elucidation of the general accuracy of the propositions, which he established regarding the contagious character of dysentery when associated with typhous fever.

This is as nearly, as may be, a short view of the contents of the work of Dr Harty as it appeared in 1805. The work was, on the whole, very favourably received; and all those who took interest in the literature of medicine, always spoke with respect of the treatise of Dr Harty on dysentery. The principal defect in the work was the small amount of information on the morbid Anatomy of the Disease. In various respects, on the other hand, the work was instructive and useful. It presented, in a short compass, the opinions and doctrines of many eminent observers and writers; and it was most useful in refuting an opinion at that time very generally entertained, without sufficient inquiry, that dysentery is, in all circumstances of climate, country, season, and locality, indiscriminately a contagious disease.*

After the interval of more than forty years, the author comes again before the profession with a second edition; and we have now to take a short view of the contents of the volume.

Dr Harty distinguishes three forms of dysentery. The first is simple dysentery; the second is dysentery in the intermittent and remittent form, or dysentery accompanied with intermittent and remittent fever; and the third dysentery, combined with contagious fever, that is the synochus and typhus of Sauvages and Cullen.

Simple dysentery it is scarcely necessary to describe. It is the ordinary form in which the disease appears in temperate countries, distinguished by griping pains of the bowels, frequent stools, mucous, serous, or bloody, and, according to Dr Harty, not always attended by fever. It is sometimes a sporadic disease; sometimes it takes place epidemically, when it is connected probably with a peculiar state of the air and a certain train of weather. The author allows that when epidemic, the cases usually exhibit symptoms of the simple disease united with fever, sometimes contagious, sometimes not.

We think that in this representation of simple dysentery being void of fever, there may be some mistake. The question, indeed, depends very much on what is meant by fever. If by this term be understood quick, strong pulse, then very often it must be allowed that fever is not present. But if by fever be meant alternate flushes and chills, that is, an irregular state of the temperature of the surface, thirst, scanty high-coloured urine, generally depositing a sediment, whether the pulse be quickened or not, and more or less coating of the tongue,—then it will be found that even simple dysentery is not unattended by fever. We have seen several times in this country simple dysentery in sporadic cases, and occasionally in more numerous cases, though not perhaps entitled to the character of epidemic; and certainly we never could say that the disorder was quite free from fever. The pulse, indeed, was not more frequent than usual, but the derangement in the functions of the skin and the glands was quite sufficient to show the presence of a febrile state. Then it is to be observed, that though the pulse be not quickened at first, as is usually the case in disorders of the mucous membranes, nor during the whole period of the twenty-four hours, yet, as the disease advances, the pulse did become quick, sometimes full and strong, and, at certain periods of the *nycthemeron*, for instance, in the afternoon and evening, and during the night, or towards morning, the pulse always became ten, twelve, or fifteen beats more rapid than at other times, and was most commonly full, strong, and a little tense.

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* Observations on the Simple Dysentery and its Combinations; containing a Review of the most celebrated authors who have written on this subject; and also an Investigation into the Source of Contagion in that and some other Diseases. By William Harty, M.B. London, Dublin, Edinburgh, 1805, 8vo. pp. 333.

It must not be denied that Willan, who describes shortly dysentery as it occurred in 1800, in London, allows that in various cases it was void of fever, and, by placing it as an epidemic of the autumn, he so far confirms the view of Dr Harty. But, on the one hand, while it may safely be referred to what is meant by the term fever, to determine whether the disease is attended by febrile symptoms or not; on the other, the occurrence of a few cases without perceptible acceleration of pulse, makes little or nothing in favour of the question. Dr Willan, though he calls the dysentery of 1800 epidemic, allows that he saw no proof of contagious propagation.

The affinity between dysentery and rheumatism next claims the attention of Dr Harty; and he quotes the authority and experience of Akenside, Richter, Stoll, Zimmermann, and Baker, to prove not only the fact that dysentery may be accompanied with, or is converted into, rheumatism, but that this conversion usually takes place when it is attempted to treat the disease by opiates, astringents, and similar repellents.

He is then led to speculate on the cause of this connection, and to inquire whether there be, in the parts affected by these two distempers, any pathological or organic relation which might be employed to explain the fact. In the course of this inquiry, he appeals to the authority of Carmichael Smyth, to show that rheumatism affects chiefly the muscular fibres, and to that of Burdin, to show that rheumatism seems to have its seat in a special manner in the white fibrous tissues, such as the articular capsules, the ligaments, the *aponeuroses*, the tendons, the tendinous *vaginæ*, and the *periosteum*. It is highly probable that this last view is the correct one; and Dr Harty thinks it may be concluded, " that membranous parts are at least as liable to rheumatic inflammation as muscular,—an opinion, he adds, strongly confirmed by the fact, that dissections have detected a viscid and gelatinous matter in the joints and sheaths of tendons in cases of chronic rheumatism, but have never exhibited any other change in the muscles, except loss of substance, arising from want of use by exercise."

The author is then led to consider shortly the morbid effects of dysentery upon the intestinal tissues, in order to inquire whether in the information thus afforded any light can be thrown on this question.

" Dissection informs us that dysentery is chiefly, if not exclusively, seated in the greater intestines, and that their inner or villous coat is the part primarily affected: but for any further information of a satisfactory nature, can we rely on dissections? What can they exhibit to our view, but the state of diseased parts after the diseased action has been able, by progressive changes of structure, to induce death; thereby depriving us of any distinct conception of that primary condition of the intestines which constituted the original disease? As 'pyrexia contagiosa' is not at least the usual agent in inducing it, we must look to some local affection of the intestines themselves for an explanation of its essential symptoms. Inflammatory action of one kind or other seems to be more or less essentially concerned in the production of the disease. Dissection shows it to have been present, often in so high a degree, as to induce death by alteration of structure, by suppuration or gangrene, and the symptoms of severe tormina, with pain on pressure, gave a similar indication. Of what kind is this inflammation? Can it be of the phlegmonous character, as dissection proves its occurrence to be the frequent cause of death? This kind of inflammation, however, usually excites high fever, and is besides attended with constipation: 'Quapropter,' says Akenside, 'inflammatio contraria videtur dysenteriæ indoli, cujus magna pars in dejectionibus nimio plus frequentibus posita est.' We may judge of the fallacy of this conclusion by subjoining to his logical enthymem the only minor præmiss which could warrant him in making it. It would run thus: 'Sed dysenteria comitatur nec febre inflammatoriâ, nec constipatione intestinorum.' Now such a præmiss would be false, for dysentery is often attended by symptoms of fever and inflammation, and it has also a species of constipation peculiar to itself, marked by the disappearance of the natural contents of the intestines, and the substitution of frequent scanty stools of mucus, blood, and serum. As simple phlegmonous inflammation will scarcely account for these peculiarities in dysentery, we cannot consider it as exerting any essential influence in the primary production of the disease."—Pp. 33—35.

This is not a very detailed account of the state of the bowels in dysentery, and, we understand, is meant to be general. There is, however, no lack of evidence on the subject; for, independent of the excellent paper by his countrymen, Dr O'Brien, there are many accounts now recorded of the state of the intestinal canal, especially the colon, in this disease; and all indicate the effects of inflammation.

The state of the intestines in dysentery has been described by Pringle, Baker, Donald Monro, Dr John Hunter, and Baillie, Cheyne, and O'Brien; and their accounts, with some trifling exceptions, in general correspond. In four dissections made by Pringle in the Flanders campaign of 1744, the villous coat of the colon was red or vascular, and abraded or ulcerated; the lower end of the colon, and generally the *rectum*, was in a state termed mortification; the ligamentous bands are said to be relaxed, half corrupted, or entirely obliterated; and the colon, sometimes the ileum and stomach, much distended by air.* In the inspections recorded by Baker of the London epidemic of 1762, the villous membrane of the rectum, colon, cœcum, and occasionally part of the ileum, was more or less reddened, velvety-granular, and occupied by numerous minute bodies like small-pox pustules, but harder and solid when divided, and fungous eminences. These hard pustules and fungous eminences were manifestly seated in the isolated follicles, which had become enlarged, infiltrated with blood, serum, and lymph, and indeed had, along with the general membrane, suffered much from the inflammatory process of the disease. In one case four or five perforations had taken place in the transverse arch of the colon.† In persons cut off by old dysentery, Monro represents the villous membrane of the rectum and colon, as inflamed with livid spots in the arch of the latter; and in one seized by violent pains of the bowels two days before death, the ileum was reddened.‡ From a subsequent account by the same author, it appears that the colic mucous membrane as high as the valve, was occupied by livid or black spots of various size, occasioned by black blood in the submucous filamentous tissue; and that in the centre of each spot there was more or less erosion of the villous membrane. Though no black spots or erosions were seen in the mucous membrane of the ileum, in one or two minute red spots, and slight traces of inflammation were recognized.* The general accuracy of these statements is briefly confirmed by F. L. Bang in the following terms. "Upon examining the internal surface of the cœcum and colon, we saw the villous coats in some parts adhering, and in others abraded; and in the same spots the vascular coat dark coloured as if filled with blood, and the mesentery reddened, very much filled with distinct vessels."†

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* Observations on the Diseases of the Army, by Sir John Pringle, M. D. London, 1768. Chap. vi.

† De Catarrho et de Dysenteria Londinensi epidemicis utrisque, anno 1762, Libellus. Auct. G. Baker, Coll. R., &c. Lond. 1764.

‡ An Account of the Diseases, &c. By Donald Monro, M. D. Lond. 1764.

Dr John Hunter, on the contrary, who states that he never saw abrasion or mortification of the villous coat, is inclined to think, that in the dissections mentioned by Pringle, the black colour arising from extravasated blood was mistaken for gangrene. Though it is impossible to doubt that this mistake has been often committed in describing the necroscopic changes of the gastro-enteric membrane, it must not be forgotten that inflammation occasionally terminates in mortification, and that instances of this are not unusual, in the tropical form of the disease especially. The general fact of inflammation is further confirmed by Maximilian Stoll, who describes the cœcum, colon, especially its transverse arch, and rectum, in persons cut off by acute dysentery, as swelled, thickened, hard, and fleshy, of a leaden or dull red colour, the mucous membrane of a foul or dingy red tint with blood, or of a deep rank green tinge removable neither by water nor the sponge,—an appearance indicating the commencement of elementary decomposition.‡

Occasionally dysentery prevails during summer and autumn in this country; and both in previous seasons and in the summer of 1843, we had opportunities of observing its effects on the intestinal coats. In several cases which were examined at that time, the following were the appearances.

The whole tract of the colon was thick, massive, and heavy, totally different from the usual membranous appearance of that bowel. This thickness was partly in the mucous membrane and partly in the submucous cellular tissue. The mucous membrane was thick, of a reddish colour, firm, and more solid than in the normal state, which was shown by the peculiar mode in which it was cut. Its surface was covered with much viscid, thick adherent mucus, which was indeed partly albuminous. The section showed that it was thickened, apparently from effusion of blood, lymph, and bloody serum into its interstitial tissue, and from the great congestion of the vessels; the colour of the substance of the membrane was also of a deeper fawn red than natural. The calibre of the bowel was much contracted, the size and capacity of the colon being not so large as that of the *ileum.*

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* Essays and Observations, Physical and Literary, Vol. iii. article 25.

† "Perlustrata interna facie intestini cœci atque coli, vidimus tunicam villosam alibi adhærentem, et alibi derasam, ibidemque tunicam vasculosam lividam quasi sanguine plenam, mesenterium rubescens vasis distinctis plenissimum."—Selecta Diarii Nosocomii Hafniensis, Tom. ii. 1786. P. 223.

‡ Ratio Medendi, Partis iii. Vol. iii. Sectionis iv.

When the mucous membrane was cleared of adherent mucus, numerous patches, variable in size from that of a pea to that of a sixpenny piece, or even larger, and irregular in figure, appeared covered with a coating of albuminous mucus tinged with the colouring matter of the bile and excrement. When these coverings were removed, they disclosed surfaces destroyed by irregular ulceration of the mucous membrane.

Those ulcers had originated in the follicles of the colon, which had become softened at the commencement of the attack, and had then proceeded to sloughing and ulceration. The state now described extended along the whole colon, but was most remarkable in the transverse arch and sigmoid flexure.

In some other cases the disease was confined to the lower part of the sigmoid flexure and to the rectum. The mucous membrane had become thickened, and similar patches of ulceration, very irregular in shape, had been formed, covered in like manner by lymph tinged with the colouring matter of the bile and excrement. The coats of the bowel were in like manner thickened and indurated, and the area of the bowel was contracted.

The necroscopic appearances of tropical dysentery have been described more or less fully by Sir W. Farquhar, Sir George Ballingall, Mr Bampfield, Mr Annesley, whose several testimonies tend to establish the general conclusion, that this disease consists in inflammation of the colic mucous membrane, spreading in general, not always or necessarily, with ulceration, but advancing to this process when not suitably or promptly opposed, and occasionally ending in death of portions of the mucous membrane. Of this inflammation the peculiarities are, 1*st*, that it is confined with considerable accuracy to the colon or large intestine, the ileum being but rarely affected, and only at its lower or colic extremity; 2*d*, that this inflammatory action spreads continuously from the ileo-cœcal valve along the mucous membrane of the *cæcum*, right branch of the colon, transverse arch and sigmoid flexure, at various rates, and with various effects, but at all times with that of producing frequent copious discharges of mucous, muco-purulent, and blood-coloured stools; 3*d*, that this process may continue for some time without producing ulceration of the mucous corion, or inflammation of the submucous tissue; that these phenomena may take place, nevertheless, in certain circumstances, at an early period; and that in others they occur towards the conclusion of the disease; and 4*th*, that though this inflammatory process in general commences with disorder of the circulation, and increased number of the cardiac pulse, it may commence without this, and almost always goes on when once established, without the pulse being much quickened, till the inflammatory process either affects the submucous tissue, or, which is nearly the same thing, begins to effect mortification of the muçous tissue.

The formation of numerous ulcers, or of pustulo-tubercular eminences in the tract of the colon, may be said to indicate the chronic form of the disorder. Ulcers, from the statements of Ballingall, Bampfield, and Annesley, appear to be more common in the tropical variety of the disorder than in that observed in more temperate climates. The same change, however, was seen by Morgagni, Lieutaud, Baillie, Cooke, and others, in the dysenteric affections of the latter description.

It is not unimportant to know that these ulcers of the intestinal mucous membrane may, under certain circumstances, undergo a process of reparation. The steps of this process, which was originally observed by Dr Donald Monro, have been well described by Petit and Serres in their account of the entero-mesenteric disease of Paris, and by Dr Latham in that of the epidemic of Millbank Penitentiary. From the observations of these authors it results, that the first step towards repair consists in the loose margin of ulcerated mucous membrane becoming fastened down to the muscular layer or the peritoneal coat respectively, by deposition of lymph all round. This lymphy deposition forms an elevated prominent ring, inclosing a depressed space corresponding to the centre of the sore, and which about the same time acquires a reticular appearance from intersecting filaments of lymph, among which may be seen minute red vessels. As the process advances, these filaments, by acquiring solidity and strength, seem to draw the mucous membrane forming the edges to the centre of the ulcer, while the elevated ring becomes flattened. At length, the lymphy deposition being covered by a thin pellicle newly formed completes the cicatrix. When the ulcerated spots are examined in this state, the ragged edges of mucous membrane are found to be mutually approximated; and the peritonæum at the same time to be puckered or drawn together, appearing as if a small portion of the intestine had been taken by the forceps and tied by a ligature. This shows that the process of repair consists not in the mucous corion being reproduced, but in the opposite margins of its breach making as it were an effort to approach, by means of the lymphy exudation from the peritonæum, which was thus necessarily contracted.

It is impossible to doubt that these pustulo-tubercular bodies are the isolated follicles, either in a state of chronic inflammation and enlargement, or infiltrated with tubercular matter, or degenerated in consequence of the long continuance of chronic inflammation. In general there are three forms under which this state presents itself. One is when the subfollicular cellular tissue becomes thickened, enlarged, and indurated; another is when this tissue is infiltrated with tubercular matter. This takes place in dysentery in those of strumous habit, and in the phthisical. A third is when the follicular membrane is degenerated and converted into a hard warty sort of matter, which usually splits or is fissured on the apex. In either of these three cases the follicles are prone to undergo a species of bad and almost insanable ulceration, causing chronic diarrhœa with great weakness and wasting.

These facts must be allowed decidedly to prove that dysentery, whatever be its remote cause, is connected with inflammation of the mucous coat of the colon, and has a great tendency to affect the muciparous follicles, which then proceed to ulceration. We are willing to concede to Dr Harty, that morbid anatomy only shows the effects of morbid action, not the cause, in all cases; and, consequently, that it is difficult, amidst so many changes of structure as dissection shows, to trace the primary action which caused all these changes. We nevertheless think that if these changes be analysed, and if they be compared in different cases, it will not be difficult to recognise the fact, that inflammation, in whatever manner produced, has been the main agent in effecting these serious changes in structure. It is, in truth, not merely to simple inflammation that we must look, but to inflammation acting on a mucous membrane, complex in structure, and liable not only to undergo complex changes, but to have these giving rise to others.

After rejecting spasm as a cause of the disease, and reducing it to its proper position as one of the symptomatic effects of inflammation of the mucous coat of the colon, Dr Harty states his final views regarding the affinity between dysentery and rheumatism in the following manner:—

> "We must, therefore, look for some other cause more adequate to the production of the several phenomena of the disease. That cause I would refer to an inflammatory action in the innumerable exhalant vessels on the mucous coat of the intestines, similar to that which takes place in serous membranes, when affected by rheumatism. Under this state of the vessels, the quantity of their secreted fluid is considerably increased, and its quality, perhaps, somewhat affected: the intestines by this stimulus are excited to spasmodic constriction, and from both these circumstances united all the other symptoms may be deduced: hence we have frequent stools with co-existing constipation; hence we have tormina with tenesmus. I have the more readily embraced this opinion, from the insufficiency of other modes of explanation, and from the facility with which this accounts for the various phenomena of the disease; indeed, the symptoms which it presents, the treatment it admits, and, in particular, its close affinity to, and frequent alternation with rheumatism, all countenance the doctrine. It explains why the dysenteric pains, which, in the language of Hoffman, "per totum

intestinorum canalem vago itinere progrediuntur," are, like the rheumatic, constantly shifting their ground, and why the dysenteric action, when disturbed in the intestinal canal by opiates and astringents, re-appears in the joints, under the form of rheumatism. It also explains why, in severe cases of the disease, whether arising from neglect or mismanagement, active inflammation is so much to be apprehended, and why, on the contrary, a chronic state so often supervenes. But whether we admit this reasoning or not, it cannot be denied that such an affinity does really exist between rheumatism and dysentery, as at least warranted Stoll in entitling them παθηματα αδελ φεα."—Pp. 37–39.

It cannot be said, however, that, admitting the existence of this affinity, the explanation of it is always easy or the same. On this point three views may be given, one of which is that espoused by Dr Harty.

First, it may be said that exposure to the same physical causes may produce both dysentery and rheumatism. Exposure, for instance, to cold, to moisture, or to malaria, unquestionably produces dysentery; and exposure to the same agents may be followed by rheumatism. In the same manner, exposure to sudden and great alternations of heat and cold may be, and we know is, followed by dysentery; and it is also matter of observation, that exposure to the same alternations, when sudden and considerable, is followed by rheumatic attacks. This appears to have been the case in many of the inst[illegible] recorded by Richter and Stoll; as in many of their patien[illegible]ldiers and labourers, the attack of either disease had been preceded by exposure to such causes, sometimes by lying on the g[illegible]d.

Secondly, In all cases of [illegible]atism, there is previously a disordered state of the circulation and secretions of the intestinal canal. The liver is often in a state of congestion, not secreting bile perfectly. The whole vessels of the intestinal canal are congested, and loaded with a mass of blood, which moves in them very slowly, and often is entirely stagnant. And it is only when this state is removed by suitable treatment, dietetic or medicinal, or both, that the rheumatic symptoms subside. It is hence, that of all treatment that by aperients is almost the only one which is in the majority of cases successful. Thus, while opiates and diaphoretics often exert a feeble and temporary influence on the disease, laxatives and purgatives, and especially those which produce a considerable secretion from the liver and intestinal mucous membrane, uniformly afford greatest and most permanent relief, and in several instances are alone adequate to the cure of the disease. Meadow saffron, when it purges most, is usually most beneficial; and even tartar-emetic solution, given as a diaphoretic and antiphlogistic, we have seen act most favourably, when it caused many liquid stools. Blue pill, in like manner, and calomel, most commonly produce their curative effects by acting as steady gentle laxatives; and it is well known that guiacum operates more usefully in that manner than upon the skin.

A similar state of the intestinal tube is constantly observed in dysentery; and it is only the imperfect and sometimes inadequate natural efforts to remove this state, that constitute the most prominent and distressing symptoms of the disease. The discharges are rarely feculent; and until they become feculent, with the characteristic appearance and sensible properties, the disease never abates or disappears. Mucus, serum, and blood, secreted from an extensively inflamed membrane, lymph effused on its surface and into its substance, show the great derangement of the whole circulation and secretion over at least 290 or 300 square inches of animal organized membrane. It is not wonderful that the retrocession or repulsion by remedies of these secretions should be followed by painful feelings in the fasciae, aponeuroses, and articular tissues.

At the same time it must not be forgotten, that at present it is a very general belief among pathologists, that in rheumatism, there is a morbid state of the blood; and probably this has not yet been ascertained regarding dysentery.

What this state is has not been ascertained. It is only generally known that the disease is attended or preceded by the formation of acid in the digestive organs—probably lactic acid—and that there is a very enfeebled condition of the action of the whole of these organs. It is possible that the dysenteric attack may pave the way for this unhealthy state of the digestive functions, and in this manner occasionally be associated with or converted into rheumatism.

Dr Harty proceeds in the third chapter to consider the second form of dysentery, or that which takes place in connection with intermittent and remittent fever.

It is unnecessary to enter minutely into the consideration of this part of the subject. It is sufficient to say that the author admits, what must be obvious to all who have studied the history of endemic diseases, that in certain situations, dysentery is the result of exposure to terrestrial or marsh miasmata; but, that as the disease is much more clearly demonstrated to proceed from exposure to cold and moisture, it is consequently more legitimate to conclude, that "as dysentery can occur in every possible relation to remittent fever, either as absent or present with it, as preceding it or consequent upon it, or as remaining after the fever has ceased, or *vice versa*,—so it cannot be likely that both are always produced by one and the same cause, more especially, as those exposed to marsh effluvia are at the same time generally exposed to wet and cold."

The rest of the chapter is occupied in considering the nosological characters of the *Dysenteria incruenta* of Willis, and the *Dysenteria colico-contagiosa* of Degner,—both of which the author adduces evidence to show were examples of cholera.

The fourth chapter introduces the reader to the main subject of inquiry in the book,—the nature of dysentery when associated with typhous or continued fever.

The author reminds his readers that, in conducting this portion of the inquiry, he has to establish two propositions, *namely*, first, that such a combination of dysentery is a real existence or actual fact; and the second, that this combination alone is contagious. The proofs necessary to establish the first proposition he arranges under two heads. Under the *first* head are considered such proofs as may be deduced from symptoms marking the presence of continued fever, and from those circumstances in the general history of each epidemic, from which may be inferred the presence of that disease. Under the *second* head are given such proofs as may be drawn from the mode of treatment suited to the different forms of dysentery. This sort of proof he defers to that part of the work in which he discusses the general treatment of the disease. The proofs of the second proposition are considered in the chapter on the contagion of dysentery.

In entering on the consideration of the first proposition, and examining the proofs which belong to the first head, the first object of Dr Harty is to prove, that in certain epidemics of dysentery, the disease is accompanied with symptoms of continued fever, and that these are symptoms of that form of continued fever, called by nosologists Typhus, or malignant fever. For that purpose he adduces the authority of Ettmuller, Rogers in his Essay on the Epidemic Diseases of Cork, Sir John Pringle as to dysentery among the troops in the low countries, Grimm in his account of the Dysentery of the town and district of Eisenach between 1759 and 1762, the account of the *Morbus Mucosus* of Goettingen in 1760–61 by Roederer and W[illegible] Sir James Macgrigor as to his experience of dysentery [illegible] troops in the Peninsula.

We are not sure that the evidence adduced from these sources shall by all be regarded as sufficient to prove the two facts,—that these epidemics were attended by symptoms of typhous fever, and that the disease was propagated by contagion. We have always entertained strong doubts, whether the Eisenach and the Goettingen visitations were anything else but telluric, or at most telluric and atmospherical diseases. The difficulty consists in this. *First,* It is quite certain that telluric influence, that is, the influence of the soil in particular districts, operated on by particular seasons and atmospherical causes, especially in the fortified towns of the continent, is quite sufficient to give rise to dysentery, and various intestinal disorders; at one time dysentery, at another diarrhœa, and at a third intestinal or gastro-enteric fever.

Secondly, It is not less certain, that the fever by which these diseases is attended is what has been denominated remittent-continuous, and is not typhous fever at all, however similar in course and symptoms it appears to that distemper. Nothing is more certain than that remittents of this kind were called continuous fevers by various physicians, especially Sennert, Morton, and several of their followers; sub-continuous by Torti; and remittents by others. Such was the semitertian of the second order, the *hemitritaeus*, and the *amphimerina*, most usually remittent, accompanying very considerable disorders in the intestinal canal. Thus Torti applies to his choleric and dysenteric-remittent, the terms sub-intrant or sub-continuous tertian fever. These remittents or remittent-continuous fevers are so similar to typhus in their latter stage, that there can be no doubt that they have often been taken for typhus, and typhus for them. The same debility of all the movements, the same oppression and derangement of the nervous system, the foul dry brown tongue, and often petechial eruptions, are observed in them as are observed in genuine typhus.

In the *third* place, it is quite certain that the intestinal fever, the glandular disease which has been [illegible] long and so confidently described as intestinal typhus, is in [illegible] not typhus at all, but a remittent-continuous fever. When [illegible] advocates found themselves obliged to admit that it was not [illegible] same as typhus, such as that disease is usually witnessed, in order to extricate themselves from the dilemma, they applied to it the name of typhoid fever, as a distinctive and appropriate appellation, to show at once its resemblance to that distemper, and to mark its actual difference from it. It is in truth the remittent-continuous which attends the peculiar disorder of the intestinal glands. Many facts concur to show, that this is not a contagious distemper, but one most frequently of telluric origin, perhaps sometimes atmospherical. Thus it prevails in [illegible] districts and localities, and not in others. It prevails where [illegible] are rivers, large or small, and river-banks; masses of stagnant water, as lakes, ponds, and canals; where there are low depressed alluvial tracts, with imperfect or no drainage, and imperfect ventilation of the surface of the soil by winds; and generally in clayey soils which are retentive of moisture.

All these considerations,—and not only might they be illustrated, but increased in number did our limits permit,—render it very doubtful whether we can always trust to the representations of authors as to this sort of fever being *typhus*. At least, after what has been witnessed in our own days in the discussion between the characters of *typhus* and typhoid fever, it would require more confidence in the powers of human diagnosis than we possess, to adopt the opinion that it is typhus. All these and similar epidemics would require to be observed again, with the full knowledge of previous mistakes, before any importance can be attached to the accounts of different observers; and as to those epidemics that are past, this is clearly impossible.

Dr Harty next gives a useful statistical account of the mortality from dysentery in London during the beginning, middle, and end of the eighteenth century, deduced from the Memoir by Dr Heberden.

> " He states that in the seventeenth century the deaths, under the title of bloody flux and griping of the guts, appear never to have been less than 1000, and in some years to have exceeded 4000, and for twenty-five years together, from 1667 to 1692, they every year amounted to above 2000: but from the beginning of the eighteenth century things were materially changed. After the year 1733, the article of griping of the guts was joined to that of the colic; if then we take the three diseases of bloody flux, colic, and gripes, their decrease is nearly as follows:
>
> " The annual average of deaths

" From the year	1700 to 1710	was	1070
	1710 to 1720	...	770
	1720 to 1730	...	700
	1730 to 1740	...	359
	1740 to 1750	...	150
	1750 to 1760	...	110
	1760 to 1770	...	80
	1770 to 1780	...	70
	1780 to 1790	...	40
" And from	1790 to 1800	...	20

> " The extraordinary disproportion of deaths in the beginning, middle, and end of the eighteenth century cannot be referred to any improper treatment of the disease, though that may formerly have added considerably to its mortality; such a circumstance cannot account for the vast disproportion of deaths in the different periods of this century, which was as follows:
>
> " At the beginning, 1100; middle, 135; end, 20.
>
> " Even in the years 1762 and 1780, when modern physicians have described the dysentery as epidemic in London, the amount of the same three articles was, in the first year, only 209, and in the last 93.
>
> " These facts relative to the gradual decline of dysentery, or rather of its mortality, in London, are not to be paralleled in the history of any other disease; the plague itself furnishes no parallel case,

because that disease either raged violently or disappeared altogether: not so the dysentery, which has declined, not so much, perhaps, in the numbers attacked, as in its positive mortality. The cause of so great an alteration in the health of the people of England, Heberden attributes to the improvements that have gradually taken place in London and all the great towns, and in the manner of living throughout the whole kingdom, particularly with respect to cleanliness and ventilation. The great influence of the assigned causes I by no means dispute; on the contrary, I am perfectly satisfied of their efficacy in the production of these happy results: in admitting this, however, I consider them to have been not the *immediate* but merely the *remote* agents in effecting such a change in the mortality of dysentery. The influence of these causes was exerted through the medium of another disease, which, if the preceding views be correct, is the great source of *danger* as well as *contagion* in dysentery: their operation was primarily exerted in mitigating and preventing contagious fever; by that means acting secondarily on dysentery, which, when separated from this its dangerous associate, is no longer an object of terror to the patient or his attendants. This opinion is supported by various facts relative to the plague, to be found in the same author. It would appear that the disease, so called, was seldom absent from London previously to the great fire of 1666, after which event it never more visited the metropolis; it appeared also that dysentery, antecedently to that period, was in general both malignant and destructive, and that shortly after there was a wonderful decrease in its frequency and severity; and though dysentery still prevailed from that time to 1692, a space of twenty-five years, it had much decreased in mortality. Morton states it to have [illegible] epidemic from 1666 to 1672, and at first exceedingly fatal (30[illegible]00 dying every week during its *acme*), though less so towards the end of that period; from Sydenham's description, dysentery appears, except occasionally, to have been devoid of contagion; and Willis's History of the Dysenteria Cruenta of the year 1670 coincides generally with this statement.

"Now I consider it almost demonstrated by Heberden, that the plague of London was nothing more than the malignant contagious fever, exalted b[illegible]ous auxiliary aids to such a pitch of destructive violence as we[illegible]it that name; and that the identity of the two diseases [illegible]orily supported by authority and facts. The singularly ra[illegible]e in the mortality of dysentery, when the plague ceased to [illegible]t London, or rather when fevers lost much of their malignity, cannot but lead us to conclude that the immediate cause of this change was owing to the diminished influence and dominion of malignant fever in London. We may indeed take it as an admitted fact, that the malignity of dysentery has ceased with the disappearance of the plague. What so common among medical writers, down to the eighteenth century, as accounts of malignant and contagious dysentery? Do not their writings abound with concurrent histories of the plague or of very malignant fevers? Diemerbroeck, for example, mentions, amongst the precursors of the plague which ravaged Nimeguen in 1636, 'Morbi epidemii mali moris, ut erant variolæ, morbilli, *dysenteriæ* valde *malignæ* et contagiosæ, imprimis febres putridæ, malignissimæ et purpuratæ, plurimisque lethales.'"—Pp. 98–101.

From all this statement we think another conclusion might have been deduced. It is certain that during the course of this very time, when dysentery destroyed regularly so great a proportion of the population, intermittent fever and remittent fever were not inactive. Indeed, from the middle, or rather the beginning of the seventeenth century, agues and bad remittent fevers were never absent from London. They proved fatal to James the First, to Oliver Cromwell, and to thousands less known. Remittents were indeed the great endemic disease of London; and from time to time they assumed those formidable characters now known only by the descriptions of Willis, Morton, and Sydenham in this country, and those of Lancisi, Torti, and Cleghorn in foreign lands.

Dysentery, then, gradually and rapidly descends from the annual mortality of 1070, among a population of 674,000 at the commencement of the eighteenth century, till it is satisfied with 20 persons at the end of that century, among a population of 1,035,500*—that is to say, its mortality is reduced more than eighty times.

Dr Harty looks for the cause of the diminished mortality, like Dr Heberden, to the improvement in the habits of the people, in the modes of living, and the more general comfort of the middling and lower classes of the inhabitants. These are, no doubt, important circumstances. But in addition to this, ague and remittent fever had become greatly less frequent and less prevalent. The margins of the Thames had been embanked, and improved by piers and wharves. Many parts of the city, both within and without the walls, had undergone such changes as could not fail to improve the health of the inhabitants. The streets, both of the city† and of Westminster, had been paved, and rendered less miasmatic. Moorfields had been built on, enclosed, and streets had been formed and paved. The Fleet ditch had been covered over, and rendered less productive of noxious vapours. Lambeth and many parts of Southwark had also shared in the general amendment. In short, the multiplied changes had effected, during the space from the beginning of the seventeenth to the end of the eighteenth centuries, a very great amendment in the health of the population, reducing the mortality, it is said, from one in twenty to one in forty persons. Now, there is not a shadow of doubt, that all this mortality was caused by the miasmatic state of London, and that it has been diminished by diminishing the causes of that miasmatic condition. It is believed also that, though there is still great room for improvement, both in the city of London and Westminster and the borough of Southwark, and also in the rural districts on the Essex and Kent sides of the river below London, yet, by considerable changes in enclosing and draining the grounds on both sides, and confining the waters of the Thames, some benefit has been conferred on the sanitary state of the population. It was the belief of many eminent observers during the eighteenth century, that the poisonous miasma of ague was brought to London by winds blowing over the alluvial surface of the Kent and Essex shores; and it is a known fact that both of these shores were formerly very productive of miasmatic vapours. There is, therefore, every reason to believe that, as both of these shores are, from agricultural improvements and other measures of amendment, now greatly less productive of these vapours, this change is one great cause of the diminished prevalence both of ague, remittent fever, dysentery, and similar maladies which are observed to be endemic in miasmatic districts.

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* It is not very easy to ascertain exactly the population of London in periods previous to the beginning of the nineteenth century. John Graunt, in his Observations on the Bills of Mortality, estimates the whole population of London, in 1661 and 1662, at 460,000. Maitland in 1760, from various sources, which he conceives to be accurate, estimates the population at 725,903 persons. Mr Rickman estimated the population in 1700 to have been 674,000, and in 1801 at 1,035,500. All these numbers agree progressively in not being excessive, and in corresponding to the usual rates at which the population of large cities increases. At the same time, they are all mere approximations.

† The streets of London were not paved till the reign of James I.

Neither does the recent history of London sickliness and mortality afford any countenance to different views. This history shows that, so far as reasoning from facts can be trusted, the same species of morbific agents are at present at work, and acquire unusual energy, as in former periods. The history of the Millbank Penitentiary diseases is a lesson that cannot and ought not in this respect to be soon forgotten. A large national prison, for the confinement and penal restraint of felons, is constructed on the north bank of the Thames, in Westminster, on ground which every person, excepting perhaps the most ignorant, knew to be alluvial, wet as a sponge, in some parts below the level of the river, in all permeable by the river water, and in certain circumstances flooded by it. Scarcely two seasons elapse, and the inhabitants of this well-chosen establishment are attacked by diarrhœa, dysentery, and scorbutic dysentery, and swept off in considerable proportions. Part of the evil is ascribed to the dietary, which was a great deal better than nine-tenths of the labourers of England and their families could procure. Part is laid to the negligence of the medical attendant. At length, after much-expensive inquiry, it appears clearly that the Penitentiary is placed in the best possible situation for insuring the ill-health of the occupants. The principal disease in this instance was diarrhœa, in consequence of inflammatory irritation of the intestinal follicles,—indeed *adeno-enteritis.*

Similar conclusions result from the facts collected regarding the prevalence of fever in London. All observers agree in representing that disease, to which they apply the name Typhus, but which we shall call simply Fever, as arising and prevailing not so much in consequence of contagion or contagious properties, as from certain localities abounding in sources of animal and vegetable decomposition. Low marshy districts, obstructed drains, unpaved courts covered with all sorts of animal and vegetable remains, and wetted with multiplied impurities, cess-pools, open ditches, lay-stalls, latrines, and the sewers of manufactories, with densely-crowded burying-grounds; these are the usually recognised sources of what is called fever in London. Contagion as contagion appears to have dwindled away to comparatively very small dimensions and to very limited operation. The disease or diseases arising, or said to arise, from the sources now mentioned, are diarrhœa and dysentery, fever, erysipelas, malignant sore throats probably of erysipelatous character, and various forms of chronic bad health, with a very disordered state of the digestive functions. We do not consider it as a deduction established on perfectly solid grounds, to say that genuine typhus is among this class of disorders. But the fact is, it is a dispute, if not about words, at least so little important, that it is almost idle to dwell on it. Nevertheless, that it may not be said, that the distinction has not been pointed out, we allow, that from the sources above mentioned fever may arise; yet not typhus, but remittent, or remittent continuous fever; and that this is the great disorder, if not most prevalent, certainly most constant, most unmanageable, and most fatal in London and its neigbourhood. This, indeed, is called the intestinal or abdominal typhus, that is, it is the foreign typhoid fever; and it is evidently the product of the soil, and the impurities and *miasmata* with which the soil may be impregnated.

We have repeatedly said, that all the facts collected regarding fever in London, show, that in that city there are prevalent two diseases to which the name of typhus is applied; the one, the ordinary genuine malignant or jail fever; and the other, the abdominal, intestinal, or follicular fever, which is not typhus, but a remittent continuous disease. The former is contagious, yet not very strongly; and its contagion is easily prevented from operating and spreading. The latter distemper, so far as can be judged from well established facts, is not contagious at all; and being a product of the soil and certain localities, affects only persons exposed to the vapours emanating from these localities. The natural insalubrity of these localities is nevertheless very much aggravated by adventitious causes connected with imperfect drainage and sewerage, accumulation of filth, and neglect of cleanliness of all kinds; and probably, by the improper construction of the dwellings of the inhabitants. As this fever is the one which attends on, or is accompanied with the follicular disorder of the intestinal mucous membrane, so is it the one which attends on dysentery and similar intestinal attacks. This brings us back to the point from which we set out, and to the work of Dr Harty.

The author considers, in the fifth chapter, the question, what evidence the writings of physicians afford to show that dysentery is propagated as a contagious disease, or as one void of that property. This leads him to give a view rather extended of the opinions and inferences formed at different times, in different countries, and as to different epidemics, regarding the alleged contagion of dysentery. The result of this investigation is presented in the following manner:—

"Having thus taken an extensive review of the chief historians of this disease, and having given a candid statement of their opinions respecting it, I shall now recapitulate their names, arranging them after the manner in which they delivered their evidence for or against the agency of contagion in its production.

"*Non-Contagionists.*—Moseley, Hunter, Willan, Akenside, Cleghorn, Sydenham, Huxham, Richter, Stoll, Rollo, Willis (?) Baker, Hillary, Milne, Somers, Fergusson.—These authors describe the simple disease, or its intermittent and remittent forms: they all with decision, greater or less, give their voice against contagion in any of these forms. Some of them deny contagion *in toto*, others are silent on the head, and some again, whilst they admit that the disease, as they saw it, was not contagious, do not deny but that under particular circumstances it may become so. This species of disagreement must the more firmly convince us of the truth of that proposition in which they all coincide, namely, *That the dysentery, in its simple, its intermittent, and remittent forms, is not contagious.*

"On an opposing list are names not less respectable:

"*Contagionists.*—Clarke, Hoffman, Ettmuller, Roederer, Grimm, Morton, Pringle, Tissot, Geach, Sennertus, Hildanus, Grainger, Rouppe, Zimmermann, Rogers, Frank, Vignes, Dewar.—These authors generally met the disease in combination with typhus; they all, with one voice and without hesitation, pronounce it contagious. Some assert contagion to be the sole, others the principal agent in propagating the disease; all, however, agree in stating, that it was contagious in the form in which it presented itself to them, namely, in combination with typhus; whence, in connexion with the preceding proposition, we must conclude, that the dysentery is contagious *only* in case of its combination with contagious fever.

"Dysentery, therefore, can no longer be deemed a contagious disease in itself, though capable of acquiring that property under peculiar circumstances. Thus we see that a contagious disease can so intimately associate itself with another, not possessed of that power, as to communicate both diseases, and not that merely which was in itself contagious."—Pp. 140—143.

The author then gives a short account of the different forms of the disease thus established,—the simple dysentery, intermittent and remittent dysentery, and the typhoid dysentery. The account of the last is all that we propose to give.

"The combination with typhus, though now happily less frequent than formerly, is by far the most important form of the disease; it agrees in every respect with Cullen's definition, and is to be distinguished by the marks characteristic of typhus fever. For, as is well remarked by Frank, 'Qui fidam typhi seu febris nervosæ gravioris imaginem a nobis depictam coram oculis posuerit, eique dysenteriæ phenomena, ut symptomata, superaddiderit, is longiore hujus dirissimi morbi (i.e. dysenteriæ asthenicæ malignæ) descriptione haud facile indigebit.' He then enumerates some of the symptoms, such as 'subita et magna vel summa mox virium vitalium prostratio, cùm pulsu exili, artuum tremore, subsultu tendinum, spasmis, somnolentia, deliriis, &c., non raro petechiæ, maculæ lividæ, nigræ, miliaria,' &c. 'In castris, in urbibus obsidione circumductis, et in classibus maritimis, nec minus in carceribus, ergastulis, strages sæpe hæc dysenteriæ species immanes, a plebe ad cives reliquos transire solita, edidisse est visa.'—'Quod si,' he adds, 'febris quæ dysenteriam malignam pro symptomate agnoscit, contagio suam originem debuerit; difficultas quoque ipsa intestinorum particeps erit contagii, nec tamen erit illis in casibus, in quibus morbi contagiosi non fuerit effectus.'

"This form is always preceded by symptoms of fever, and its access is generally attended by rigors, nausea, vomiting, and *great prostration of strength.* Its progress is accompanied by the more advanced symptoms of the same fever, and its termination, too often unfavourable, is marked by all those which are usually present in typhus, the fatal termination and crisis falling on the same days as in that fever; and in every respect, says Grimm, 'Ratio vitam ponendi omnibus ferè eadem, ac in febribus malignis fuit.' This form is exceedingly dangerous, and its more immediate danger generally proportioned to and dependent on the severity and bad type of the fever: it is propagated solely by contagion ('licet causæ procatarticæ,' as Van Swieten states, 'non adfuerint manifestæ'), and in its mode of propagation resembles fever. Even when terminating more favourably, by the subsidence of the fever, the dysenteric affection may and does generally continue more or less, to the no small danger of the patient: the contagious property of the disease, however, I should think, ceased with the influence of the fever, as upon that it originally depended.

"Of the more immediate and remote danger attending its visitation, ample testimony is furnished by Willis, Vignes, Cheyne, and sundry others. 'Dysenteria,' says Willis (cap. x. de Febribus, p. 121), 'est effectus in febribus continuis adeo frequens ut quibusdam annis epidemius evadat, et peste non mitior, plurimos interimat.' Vignes, who encountered the disease in all its forms during the French campaigns in Spain and Germany, gives many interesting and minute details on the subject generally, and on the mortality specially. When the disease was severe and complicated, and the condition of the soldiers unfavourable, there often perished from one-third to one-half of the sick. On one occasion, speaking of the employment of warm baths in malignant dysentery, he cautions against the free use of them for fear of fainting, and adds, 'les trois varietès de la dysenterie maligne sont en general si graves, qu'il y a tres peu d'espoir de sauver les malades.'

The details furnished by Cheyne of the dysentery which prevailed in Ireland, concurrently with the contagious epidemic fever of 1817 and 1818, are highly instructive on many points connected with this combination of disease, and exhibit specimens of the various phases it may assume. according to the character of the fever. It would appear that the disease manifested itself in different places at different periods and at different seasons of the year, being very prevalent and fatal, and in general deemed contagious. At Cork, one in three of those attacked was the estimated mortality. From all the statements he had received, and they were numerous and authentic, Cheyne was disposed to conclude that 'the inhabitants of Cork were liable to dysentery in each of its principal varieties, both as connected with continued fever and with intermittents: the former varieties contagious; the latter not at all so.' And further, that the dysentery of 1818 'rather belonged to that great variety of dysentery which originates in causes that are also productive of continued fever.'

'Cheyne's accurate delineation of the epidemic, as it appeared in Dublin, is, that it 'very often arose *during convalescence* from fever, and in many instances he ascertained that the preceding fever was not attended by any unusual gastric irritation. Sometimes it commenced at the termination of fever, and frequently it arose in the course of fever,' but, from the peculiar nature of the fever of the season, it was sometimes difficult to mark the precise time when the new disease commenced. The fever having, during the prevalence of dysentery, in place of extending to the end of the second and third week, assumed a duration of five, seven, and nine days, and then very liable to one, two, or more relapses, it was observed that dysentery frequently occurred at that period of recovery from fever when there seemed the greatest liability to relapse; and that, whether in connection with fever or not, it often commenced with a rigor, and terminated in free perspiration; that it was sometimes converted into a fever, while, on the other hand, fever was frequently converted into dysentery. In short, these forms of disease were convertible the one into the other, so that the opinion of Sydenham, that dysentery is a *febris introversa,* received support from our observations.' He further remarked, that 'the most severe cases in the *first* stage, and the most unmanageable throughout, arose in the course of fever; the mildest cases were those which seemed to have the *least* connection with that disease.' At one period, a majority of the patients died who had been ill more than six or seven days, and, of those who were emaciated, scarcely one recovered. 'In some cases, the disease hurried to a fatal termination with a speed which nothing could retard; and sometimes a case, at first marked with no symptoms of unusual danger, suddenly changed its character to that of uncontrollable severity.

"The great prevalence of the contagious form of dysentery is not limited, like the last, to the autumnal season, though the combination may generally originate at that period of the year. It extends its ravages to the winter months, being dependent not so much on the seasons as on the febrile contagion, and continues so long as the causes of that fever continue to operate. The last quotations from Frank and Van Swieten, and Cheyne's statement, strongly illustrate these positions, diversified though these may be by the peculiarities of the original fever. That fever may, as we have repeatedly proved, be either originally a malignant typhus, or it may have been a remittent of bad character, converted by the peculiar circumstances of the sick into one of a contagious continued type. Such a change, though doubted by Bancroft, is too well authenticated to be called seriously into question. 'Nous ferons remarquer,' says Vignes (p. 365), 'que l'infection d'une maladie ne produit pas toujours la meme espece d'affection. L'infection de la dysenterie peut, par exemple, occasioner une indisposition bilieuse ou une fievre adynamique,' &c., 'de meme qu'on peut etre affecté de celle-ci (la dysenterie) par toute autre exhalaison dèletère que celles des dysenteriques.'

"If there be any truth in the statements now submitted to the reader, from authorities ancient and modern, and from observers in every clime, the importance of the distinctions I have proposed for his consideration must be obvious. In *theory* they are so far satisfactory as they furnish a ready solution of the many difficulties and contradictions we meet with in the best histories of the disease. But it is in a *practical* point of view that they possess the strongest claims on our attention. By them we are admitted to a clear and, I would hope, a correct view of all its varieties; and, no longer embarrassed by uncertain opinions and indecisive practice, we are led in one case not to entertain unnecessary fears of contagion, or in another to deride them as unfounded; but are thereby taught when to consider precautions requisite, and when to reject them as needless. These distinctions, too, will be found to constitute our best guide to a safe prognosis and a sound practice. The truth of this latter position will be amply exemplified in the subsequent chapter, which may also satisfy us of the correctness of Rush's remark that 'as the discovery of truth in religion reconciles the principles of opposite sects, so the discovery of truth in medicine reconciles the most opposite modes of practice.' "—Pp. 147–155.

After the observations we have already submitted on the general question, we propose to leave the whole matter to the examination and judgment of our readers. Our own views we do not desire to impress further or more forcibly, than the facts may truly justify. As to those given by Dr Harty, we must allow that they are plausible, and possess a great amount of verisimilitude. The question is one not by any means easy of determination; and all that can be said is, is it necessary to have recourse to contagion to explain the phenomena of the propagation of dysentery. If the views which we have already given be accordant with fact, it seems not necessary. This, however, we allow to be matter of opinion. It is one advantage of such a work as that by Dr Harty, that it furnishes the reader with the requisite information, instructs him in the difficult duty of balancing evidence, and insensibly thereby enables him to form conclusions deduced from multiplied though perhaps rather discordant facts.

In the sixth chapter, on the Treatment of Dysentery, Dr Harty, taking an extensive view of the several remedies employed, as evacuants or antiphlogistics, tonics, sedatives, and counter-irritants, adduces from the history of each further arguments in favour of his doctrine, that dysentery with typhous fever is the only contagious form of the disease.

He then in chapter seventh, gives a view of the treatment most suited to each of the forms of the disease; and his observations on the treatment of the third form, which, indeed, is only the advanced or asthenic stage of the second form of the disease, may be perused with interest.

The eighth chapter, which is also the last, is devoted to the examination of the question, what evidence is afforded to show whether malignant angina, rose, hospital gangrene, and puerperal fever are at certain periods, and in certain seasons, propagated in the manner of contagious diseases from one person to another? All these disorders, Dr Harty sees reason to believe, are, in certain circumstances, propagated by contagious influence,—in fact, by the agency of some poisonous material. Into the consideration of this question it is unnecessary here to enter. Different opinions have been and will be entertained on the contingent contagious character of these distempers by different physicians; and we cannot conceive it possible to decide the question in one mode or other. In some instances, these distempers appear as atmospheric diseases, that is dependent on some state of the air. In others it is possible that they may acquire a property of contingent or accessory contagion. But whether they do or do not, the facts observed on each side are so equally balanced, that the more they are examined, the more remote from determination they appear to be. To all who wish either to obtain information, or to have their doubts removed, we can justly recommend the work of Dr Harty, as one illustrated by learning, and profitable, by the judgment and reasoning which it exhibits.

[APPENDIX TO ORIGINAL COMMUNICATIONS.]

ART. VIII.—*Case of Aneurism, and operation of tying the External Iliac Artery, terminating favourably.* By ROBERT COCKS, M. D., Surgeon, Dundee.

ON the 31st October last, 1847, a message was left at my surgery, requesting me to visit a person named Alexander Walker, living in the suburbs of the town. I saw him the same evening, when I found him in bed. On inquiring into the history of his case, he stated that he had all his life enjoyed uninterrupted good health, with the exception of a syphilitic complaint, which he contracted about nine years ago, and for which he underwent a course of mercury. He said he was 46 years of age, married, and of sober habits; that he had been engaged for the last six years as a labourer in the gas work, where his duties were very arduous and fatiguing; but notwithstanding he enjoyed good health, and his strength continued unimpaired.

He informed me, that, two months previously, he first felt a pain in his right ham, which extended down the back of the leg, and caused great weakness in the whole of the extremity, and that a fortnight ago, he for the first time observed a pulsating tumour, about the size of a hen's egg, underneath his right groin, which rapidly increased, and that *pari passu* with its increase, the pain and weakness of the leg became more and more aggravated. He stated that, four days ago, the pain in the limb assumed such an intensity as to confine him to bed, and that it still continued to distress him very much.

On examining the right thigh, I found a circumscribed pulsating tumour in the course of the femoral artery, immediately before it entered the tendon of the *triceps* muscle, about the size of a large orange, and which separated, to a considerable extent, the *sartorius* and *rectus* from their natural position. On the surface of the tumour were three protuberances, each being about the size of a sixpenny piece, very elastic to the touch, and through which the pulsation was peculiarly marked, so distinctly, indeed, as to convey a painful impression, that at each successive pulsation the sac would give way, and its contents suddenly burst forth. With the exception of these spots, the tumour, when compressed by the hand, conveyed to the fingers a sensation as if it were a *doughy* substance; and a dull pulsation was felt all over it. By compressing the artery, immediately above the tumour, so as to cut off the supply of blood from it, the pulsation at once ceased, and the tumour became flaccid and shrunk in appearance; but the moment the pressure was removed, it speedily resumed its former shape. Handling the tumour even rather roughly, appeared to give the patient no pain or uneasiness. The skin was nowhere discoloured, neither was there any œdema of the leg.

The contents of the visceral cavities appeared to be in a healthy state; his pulse was calm and natural, and his appetite good.

The character of the tumour being so distinctly marked, I could have no doubt about the nature and cause of my patient's malady, and contented myself with prescribing for him five grains of blue pill at bed-time, and the usual black draught in the morning, as a preparatory step.

The following day I requested a medical friend to visit the patient along with me, who, after examining the tumour, at once concurred in my views, and suggested that no time should be lost in operating, as he feared that one of the spots on its surface might give way, which would not only endanger the patient's life, but at least render the operation much more difficult. The following evening I held a consultation with another medical friend on the case. He, too, came to the same conclusion, and repudiated trusting for a cure by compression, as wilfully hazarding the patient's life. My own opinion being thus strongly confirmed by those of my respected friends, I resolved, with their entire approbation, to operate the following morning.

We met next morning at eleven A. M., when I was delighted to find that my patient had passed a comfortable night, and that his mind was free from any apprehensions about the operation. He was brought from bed, placed on a table, his shoulders being elevated by pillows, his legs hanging over the end of it, and his feet supported on a chair. Thus ready, we administered to him the vapour of sulphuric ether, which he very reluctantly consented to inhale, courageously protesting against being inspired with "Dutch courage." By a little persuasion his prejudice was overcome, and we succeeded in making him inhale it for a considerable time, but without being able to induce a sufficient degree of anaesthesia.

I began the operation by making a longitudinal incision through the integuments and superficial fascia, of about four inches, in a direction nearly parallel with that of Poupart's Ligament, and about an inch above it. This brought to view the tendon of the external oblique, which I divided in like manner; a few fibres of the internal oblique and transversalis were cautiously raised and separated, exposing the *fascia transversalis.* A director being introduced, these muscles were freely divided to the extent of the external incision. The artery being still at a great depth, it was necessary to employ flexible copper-retractors to separate the edges of the wound. The fascia was then gently separated, the *peritonæum* raised, and the finger introduced down to the vessel; a little counter pressure being made with the finger, the aneurism needle was cautiously insinuated under the artery, the convexity of the instrument being turned towards the peritonæum. Slight pressure on the needle, with the vessel interposed, completely prevented all pulsation in the aneurism. The ligature being firmly tied, both ends of the silk were left hanging out of the wound. The artery, at the point where the ligature was applied, appeared to be perfectly healthy.

The edges of the wound were approximated, retained by three stitches, and light-water dressing applied. Little blood was lost during the operation; the only vessel divided was a branch of the *circumflexa ilii*, which was immediately secured by ligature. The limb was then wrapped in flannel, and the patient placed in bed. He complained of no pain or uneasiness, only of a chilly sensation in the right limb.

At six P. M. he complained of pain in the situation of the ligature. The temperature of right limb natural; pulse 84, full and soft; skin moist; no pulsation in the tumour.

Nov. 5, ten A. M. He passed a favourable night; slept two hours; complains of little uneasiness; pulse 84; tongue clean; slight thirst.

Four P. M. The patient has slept several hours since morning. Pulse 84, soft; skin moist; the temperature over the whole body of the natural standard; thirst gone. Had bread and milk for dinner.

6th. Slept several hours during the night; the dressings were partially changed, and union by the first intention has taken place over the whole extent of the wound. Pulse 84; tongue clean; temperature of right limb natural.

7th. Passed a good night; pulse 72. In other respects the same as yesterday; appetite good.

8th. Slept well during the night. Pulse 66; tongue whitish; appetite less keen than yesterday; bowels not relieved since the operation.

To have immediately seven drachms of castor-oil.

9th. Castor-oil operated well, and feels more comfortable than yesterday. Pulse 76; tongue clean; temperature natural. One of the stitches removed; wound quite adherent; aneurismal tumour considerably diminished, and experiences no difference in the right limb from the left.

11th. The patient complains of no uneasiness. Pulse 60, full and soft; tongue clean; bowels moved to-day without medicine. He slept several hours last night. The other sutures removed, and the incision completely cicatrized.

15th. He continues to proceed favourably. Pulse 60; appetite good; bowels were moved naturally every alternate day since last report. Has only been allowed the lightest food since the operation; wishes a little fresh animal broth to-morrow. Let him have it.

19th. Regains his usual strength; feels comfortable, and has no complaint. Aneurismal swelling much diminished; ligature firmly fixed in the wound. Pulse 56; tongue clean; bowels moved daily since last report; and the patient states that he sleeps much the same as when in health previous to the operation.

30th. Continues to improve; the ligature has not separated.

Dec. 20th. The ligature remains firmly adherent in the wound. On applying a slight degree of force to it, the silk broke under the skin, leaving the knot deeply imbedded in the wound. The aneurismal sac is reduced to one-half its former size. The patient has been so careful and anxious to promote the cure, that no persuasion could induce him to allow his bed to be made until to-day. When standing in the floor of the apartment, he stated that the right limb was weaker than the left, but in every other respect the same; health improving, and regains strength.

30th. The patient is out of bed the greater part of the day, and walks about the house without difficulty; requires an occasional purgative.

Jan. 4th 1848. Walked out of doors.

15th. Walked several miles to-day; still feels the limb weak. In every other respect well. The sac has nearly disappeared.

March 1st. The patient expresses himself well, and is again able to work, but still afraid to resume his former employment, lest it prove too heavy and laborious for him. He appears to me to be as well as ever I saw him. No perceptible difference is apparent between the right limb and the left, and the sac has completely disappeared. On careful examination, a hard cord is alone distinguishable in the situation of the femoral artery.

7, *Bain Square*, *Dundee*,
16*th March* 1848.

PART III.

MEDICAL INTELLIGENCE.

MEDICAL STATISTICS AND PATHOLOGY.

Travels in Peru during 1838-1842, *on the Coast, in the Sierra, across the Cordilleras and the Andes, into the Primeval Forests.* By Dr J. J. Von Tschudi.—The west coast or pacific side of the South American continent, and especially Peru, possesses, in various respects, a climate better suited for preserving the health of Europeans threatened with, or labouring under, diseases of the lungs, than most of the places usually recommended either in the old or the new world. Though situate within the tropics, its elevation above the level of the sea renders it much more temperate than other equinoctial regions. Besides this character, the surface is protected part of the year from the direct rays of the sun by a veil of thin vapour or fog, which, stretched through the atmosphere like an awning, allows sufficient light and heat to descend to the earth without causing that burning temperature found in central Africa, southern Asia, and various islands of the West Indies. The south and southwest winds also tend to keep the temperature moderate.

This thin vapour or mist, which is denominated *garua*, differs from that which forms clouds in this respect, that it allows the sun and stars to be seen through it, that it is not accumulated in irregularly-shaped and variously coloured dense masses like clouds, and that it seldom or never is converted into rain; actual rain being an occurrence so rare in Peru, that, when it takes place, it is noted as an extraordinary event. It only dissolves into a sort of dew. The physical causes of this attenuated vapour are not well known. But they appear to be connected with the considerable elevation at which the winds from the South Pole and southwest meet the surface of the earth on the west side of the Andes.

But, whatever be the cause, it seems certain, from various authentic testimonies, that Lima and various of the mountain residences in the neighbourhood, and in Peru in general, are favourable to the restoration of persons labouring under feeble or diseased lungs and several other morbid states, which are aggravated either by a very hot or a very cold climate, or one liable to great vicissitudes. The most recent observer, Dr Von Tschudi, has communicated several interesting facts regarding the climate of Lima and other places in Peru; and it may be useful to have his testimony on various points in relation to the most common and severe complaints. In several respects he has been anticipated by Dr Archibald Smith, in his paper on the Diseases of Peru, in the 53d, 54th, 56th, 57th, and 58th volumes of this Journal. But on certain points he gives fuller details.

Situation and Diseases of Lima in Peru—Lima, according to the careful observations made by Herr Scholtz, is situated in 12° 3′ 24″ south latitude, and 77° 8′ 30″ west longitude from Greenwich. It may, however, be mentioned that the longitude from Greenwich is very differently stated. In sea charts and manuals of geography it is often marked 76° 50′. Humboldt makes it 77° 5′ 5″; and Malaspina 77° 6′ 45″. According to Ulloa, it is 70° 37′ west of Cadiz. The latitude is very generally fixed at 12° 2′ 3″ south. The height above the level of the sea is also differently estimated. Rivero, in the *Memorial de Ciencias Naturales*, I., 2, page 112, states it to be 164 metres, or 462 French feet. On another occasion he makes it $184\frac{4}{5}$ Castilian varas (each vara being equal to 33 inches English). He gives the following account of heights, according to the barometer, between Callao and Lima, in varas, viz. Callao, 00: Baquijano, $24\frac{3}{5}$; *La Legua*, $50\frac{3}{8}$; Mirones, $94\frac{3}{10}$: Portada del Callao, 150; *Plaza de Lima*, $184\frac{4}{5}$.

The first estimate given by Rivero is the most correct. Gay makes the height of Lima, at the corner of the church of *Espiritu Santo*, 172·2 Castilian varas; but most of his heights are incorrectly stated.

The conical hill in the north-east of Lima, called Cerro de San Cristoval, is, according to trigonometrical measurements, made in 1737, by Don Jorge Juan, and de la Condamines, 312 varas higher than the Plaza Mayor, or 134 toises above the sea; but one of the most exact measurements is Pentland's, who found the height to be 1275 English feet.

The average temperature during the hottest period of the year, from December to March, is 25° C. The medium temperature during the cold season, from April to November, 17·5° C. Highest rise of the hygrometer, 21·5°.

The low temperature of Lima at the distance of only twelve degrees from the equator is to be ascribed to the situation of the town, and the prevailing atmospheric currents. The Cordilleras, rising at the distance, only twenty-eight Spanish leagues east of the city, are crowned with eternal snow; and on the west the sea is distant only two leagues. The prevailing wind blows from the south-south-west. West winds are not very common, though they sometimes blow with extraordinary violence for those regions, and breaking on the surrounding mountains, they form atmospheric whirlwinds, which diffuse alarm through the whole population. In June 1841, I had the opportunity of observing one of these dreadful whirlwinds, which swept away huts, and tore up trees by the roots. The atmospheric currents from the north, which pass over the hot sand-flats, are not of constant occurrence, but they are oppressively sultry. There must be other causes for the low temperature of Lima, for in the villages, only a few miles from the city, and exposed to the same atmospheric influences, it is much higher.

Miraflores is a small place, about one Spanish league and a-half from Lima, but it is much hotter. Among the records of the thermometer are the following:—

December 20 to 27, maximum, 31·8° C.; minimum, 25·9° C.

December 28, at 6 in the morning, 26·0° C.; at 2 P. M., 32·7° C.; at 10 at night, 27·3° C.

January 1, at 2 P. M., 33·1° C., maximum of the day.

January 18, at 2 P. M., maximum 34·2° C.

A comparison with the temperature of Lima, on the same days, gives an average of 5·7° C. of heat in favour of Miraflores.

The river Rimac, which rises among the glaciers of the Cordilleras, and after a course of no great length, intersects the city, doubtless contributes to cool the atmosphere.

The climate of Lima is agreeable, but not very healthy. During six months, from April to October, a heavy, damp, but not cold mist overhangs the city. The summer is always hot, but not oppressive. The transition from one season to another is gradual, and almost imperceptible. In October and November the misty canopy begins to rise; it becomes thinner, and yields to the penetrating rays of the sun. In April the horizon begins to resume the misty veil. The mornings are cool and overcast, but the middle of the day is clear. In a few weeks after, the brightness of noon also disappears. The great humidity gives rise to many diseases, particularly fevers, and the alternations from heat to damp cause dysentery. On an average, the victims to this disease are very numerous. It is endemic, and becomes, at apparently regular but distant periods, epidemic. The intermittent fevers or agues, called *tercianos*, are throughout the whole of Peru very dangerous, both during their course and in their consequences. It may be regarded as certain that two-thirds of the people of Lima are suffering at all times from *tercianos*, or from the consequences of the disease. It usually attacks foreigners, not immediately on their arrival in Lima, but some years afterwards. In general the tribute of acclimatization is not so soon paid by emigrants in Lima as in other tropical regions.

The population of Lima has at various periods undergone great fluctuations. In the year 1764, the number of inhabitants was stated to be 54,000; in 1810, 87,000; in 1826, 70,000; in 1836, 54,600; and in 1842, 53,000. The accuracy of all these estimates is questionable. All that is known is, that the population of Lima has considerably decreased since the declaration of independence. Several parts of the city are now totally uninhabited.

In the tax registers, drawn up during the protectorate of Santa Cruz in 1836, the number of the inhabitants of Lima is represented as follows :—

	Male.	Female.	Total.
1. White Creoles, ...	9,423	10,170	19,593
2. Indians, ...	2,561	2,731	5,292
3. People of colour (mixed races),	11,771	12,355	24,126
4. Slaves, ...	2,186	2,606	4,792
5. Ecclesiastics, ...	475	359	825
	26,416	29,212	54,628

In consequence of the ignorance of the medical attendants, and the neglect of the police, the statistical tables of deaths are very imperfectly drawn up, and therefore cannot be entirely depended upon. I may, however, here subjoin one of them, which will afford the reader some idea of the mortality of Lima.

The annual number of deaths in Lima varies from 2,500 to 2,800.

In the ten months, from the 1st of January to the 30th of October 1841, the number of marriages was 134, of which 46 were contracted by whites, and 88 by people of colour.

Deaths in Lima from January 1, *to October* 30, 1841 :—

Diseases.	Men.	Women.	Children.	Total.
Dysentery, ...	171	105	59	335
Fevers, chiefly intermittent, ...	57	88	71	216
Typhus, ...	14	7	24	45
Pulmonary consumption, ...	87	110	11	208
Inflammation of the lungs, ...	78	75	26	179
Dropsy, for the most part a consequence of intermittent fevers, ...	33	32	7	72
Hooping-cough,..	...	...	36	36
Small-pox, ...	3	...	1	4
Sudden death,...........................	23	13	1	37
Shot, ...	3	...	...	3
Various diseases,...	271	228	61	1109
	740	658	846	2244

The number of births were :—

	Boys.	Girls.	Total.
In marriage, ...	410	412	822
Not in marriage, ...	432	428	860
	842	840	1,682

The number of births not in marriage (860) is remarkable, and no less so is the number of dead children exposed, which, during the above interval, was 495. These are most decided proofs of the immorality and degraded state of manners prevailing in Lima, particularly among the coloured part of the population. Though there is no certain evidence of the fact, yet there is reason to conjecture that a considerable number of those infants are destroyed by the mothers. Of the children born out of marriage, nearly two-thirds, and of those exposed dead, full four-fifths are Mulattos.

The important annual surplus of deaths over births is a matter of serious consideration for Lima. The above tables show, in the course of ten months, a surplus of 562 deaths. By a comparison of the lists of births and deaths from 1826 to 1842, I find that on an average there are annually 550 more deaths than births. It would lead me too far to endeavour to investigate all the grounds of this disparity, but I may observe that one of the causes, unquestionably, is the common, though punishable crime of producing abortion.

Along the whole coast of Peru the atmosphere is almost uniformly in a state of repose. It is not illuminated by the lightning's flash, or disturbed by the roar of the thunder: no deluges of rain, no fierce hurricanes destroy the fruits of the fields, and with them the hopes of the husbandman. Even fire appears here to have lost its annihilating power, and the work of human hands seems to be sacred from its attack. But the mildness of the elements above ground is frightfully counterbalanced by their subterranean fury.

Lima is frequently visited by earthquakes, and several times the city has been reduced to a mass of ruins. At an average, forty-five shocks may be counted on in the year. Most of them occur in the latter part of October, in November, December, January, May, and June. Experience gives reason to expect the visitation of two desolating earthquakes in a century. The period between the two is from forty to sixty years. The most considerable catastrophes experienced in Lima since Europeans have visited the west coast of South America, happened in the years 1586, 1630, 1687, 1713, 1746, 1806. There is reason to fear that in the course of a few years this city may be the prey of another such visitation.

The slighter shocks are sometimes accompanied by a noise; at other times they are merely perceptible by the motion of the earth. The subterraneous noises are manifold. For the most part they resemble the rattling of a heavy loaded waggon, driven rapidly over arches. They usually accompany the shock, seldom precede it, and only in a few cases do they follow it; sounding like distant thunder. On one occasion the noise appeared to me like a groan from the depth of the earth, accompanied by sounds like the crepitation of wood in partitions when an old house is consumed by fire.

Of the movements, the horizontal vibrations are the most frequent, and they cause the least damage to the slightly-built habitations. Vertical shocks are most severe; they rend the walls, and raise the houses out of their foundations. The greatest vertical shock I ever felt was on the 4th of July 1839, at half-past seven in the evening, when I was in the old forests of the Chanchamoyo territory. Before my hut there was an immense stem of a felled tree, which lay with its lower end on the stump of the root. I was leaning against it and reading, when suddenly, by a violent movement, the stem rose about a foot and a half, and I was thrown backwards over it. By the same shock the neighbouring river, Aynamayo, was dislodged from its bed, and its course thereby changed for a considerable length of way.

I have had no experience of the rotatory movements of earthquakes. According to the statements of all who have observed them, they are very destructive, though uncommon. In Lima I have often felt a kind of concussion, which accords with that term in the strictest sense of the word. This movement had nothing in common with what may be called an oscillation, a shock, or a twirl. It was a passing sensation, similar to that which is felt when a man seizes another unexpectedly by the shoulder, and shakes him; or like the vibration felt on board a ship when the anchor is cast, at the moment it strikes the ground. I believe it is caused by short, rapid, irregular horizontal oscillations. The irregularity of the vibrations are attended by much danger, for very slight earthquakes of that kind tear away joists from their joinings, and throw down roofs, leaving the walls standing, which, in all other kinds of commotion, usually suffer first, and most severely.

Humboldt says that the regularity of the hourly variations of the magnetic needle and the atmospheric pressure is undisturbed on earthquake days within the tropics. In seventeen observations which I made during earthquakes in Lima with a good Lefevre barometer, I found, in fifteen instances, the position of the mercury quite unaltered. On one occasion, shortly before a commotion, I observed it 2.4 lines lower than it had been two hours before. Another time I observed, also on the approach of the shock and during the twelve following hours, a remarkable rising and sinking in the column. During these observations the atmosphere was entirely tranquil.

Atmospheric phenomena are frequent, but not infallible prognostics of an earthquake. I have known individuals in Lima, natives of the coast, who were seldom wrong in predicting an earthquake, from their observation of the atmosphere. In many places great meteors have been seen before the commotion. Before the dreadful earthquake of 1746, there were seen fiery vapours (*exhalaciones encendidas*) rising out of the earth. On the island of San Lorenzo these phenomena were particularly remarked.

Many persons have an obscure perception—a foreboding, which is to them always indicative of an approaching earthquake. They experience a feeling of anxiety and restlessness, a pressure of the breast, as if an immense weight were laid on it. A momentary shudder pervades the whole frame, or there is a sudden trembling of the limbs. I, myself, have several times experienced this foreboding, and there can scarcely be a more painful sensation. It is felt with particular severity by those who have already had the misfortune to have been exposed to the dangers of an earthquake.

Unhealthy climate of town and vicinity of Huaura.—In a fine valley, about two short leagues from Huacho, the little town of Huaura is situated on the bank of a river of the same name. This Rio de Huaura is formed by the union of two rivers. The larger of the two rises in the Cordillera de Paria, and flows through the wild ravine of Chuichin: the smaller river, called the Rio Chico de Sayan, rises from a lake of considerable size in the Altos de Hauquimarca. Both unite below the village of Sayan. In the vicinity of Huaura the river forms several marshes, in which malaria is generated. In very few places have I seen the stratum of malaria so distinctly separated from the atmosphere as here. It lies at an average about two, or two and a half feet above the marsh, and is carried over it by strong atmospheric currents. It is distinguished by a peculiar kind of opalization, and on certain changes of light it exhibits a yellowish tint. This is particularly perceptible in the morning, on coming down from the high grounds. The marshy plain then appears overhung with a thick colour-changing sheet of malaria. Malignant intermittent fever and diseases of the skin are frequent in Huaura. The town is thinly peopled; the number of inhabitants being not more than 2000.

A great sugar plantation, called El Ingenio, is situated at about a quarter of a league from Huaura. It formerly belonged to the Jesuits, but is now the property of a rich Lima family. The *trapiche*, or sugar-mill, is worked by a water-wheel, the first ever established in Peru, a circumstance of which the owner proudly boasts.

The valley which opens here is magnificent, and to ride through it easterly eleven leagues towards Sayan is one of the finest excursions which can be made in Peru. Over this beautiful district are scattered many rich plantations. The one next in importance to El Ingenio is Acaray, which, though not very large, is most carefully cultivated: another, called Huillcahuaura, has a splendid building erected on it. In the middle of the valley is the extensive sugar plantation of Luhmayo.

* * * *

Next morning, the person who officiated as medical superintendant of the plantation showed me all the arrangements of the establishment. He gave me an account of his cures and operations, and told me that he often found it necessary to amputate, because the slaves purposely injure their fingers and arms in the *Phalangeles* (machines) in order to disable themselves. The worthy Æsculapius had never in his life read a regular medical work. He had originally been an overseer of slaves, and had afterwards turned doctor. He informed me that, some time before I saw him, ninety negroes, his patients, had died of small-pox in the space of nine months, whereby the owner of the plantation had lost 45,000 dollars. The hospital was clean and well-fitted up, but over-crowded with sick. Most of them died from intermitting fever, and from dropsy and rheumatism which followed it. Not a few of the male negroes suffer from a peculiar kind of cutaneous disease, which shows itself by large pustules on the arms and breast. After suppuration they dry and fall off, but leave indelible spots, which, on a black skin, are of a whitish colour; on a brown skin, olive-green; and on a white skin, black. I never saw the disease in any other part of the country except in this valley. Negroes and persons of mixed blood are more subject to it than the whites.

This disease is called by the inhabitants Caracha.

The fruitful valley of the *Pasamayo* contains two villages and eighteen plantations. In no other valley are there so many earth-fleas, or *piques*, as they are called, particularly about the plantations. The *pique* is a small white insect, which lives in sand, but fastens as a parasite on man and beast, more particularly on swine. It attacks man by penetrating the skin, for the most part under the toe-nails, where an egg is laid, from which a painful tumour is afterwards formed. Should this be neglected, the brood is developed, and penetrates further into the flesh. Then follow violent inflammations and imposthumes, which sometimes assume so serious a character, that the amputation of the foot becomes necessary. While the *pique* is penetrating there is no sensation of its presence; it is first felt on the development of the egg, and then it is still easy to remove the bag which contains it, and the mother with it. The Negresses accomplish this with great dexterity. They make an aperture in the skin by scratching it with a needle, and then they draw the bag out. Should it burst, they take out the egg with the needle; but this is a very delicate operation. I have always been able to do it more speedily and more securely with the lancet. The hole is commonly of the size of a bean, and hot cigar ashes are put into it to destroy any eggs or larvæ which may remain. These insects do not always confine themselves to the feet; they sometimes attack the body and the face, and it is in general extremely difficult for the patient to discover how or where he became acquainted with such troublesome companions. I once had six tumours, caused by broods of *piques*, on my right foot, and I could not trace the annoyance to any other cause than having stopped for a few minutes, while my horse was being saddled, in the *corral* or yard of a plantation.

Verrugas, an eruptive disease prevalent among the inhabitants of Peru. The Uta, a species of Cancer.—In several of the valleys on the road from the coast to the Sierra, and above all in the valley of Surco, there are certain springs, the water of which the Indians never drink. When a stranger unguardedly approaches one of these springs for the purpose of quenching his thirst, he is saluted by warning cries of *Es agua de Verruga!* (It is verruga water!) Even horses and mules are not suffered to refresh themselves at these springs, where the water is supposed to have the effect of producing a disorder called the *verrugas*. As the existence of this disease is not known in any other country, there appears ground for believing that it has its origin in certain local circumstances. The verrugas first manifests itself by sore throat, pains in the bones, and other feverish symptoms. In the course of a few days an eruption of red-coloured pimples or boils appears. These pimples sometimes increase in magnitude, till, in some parts of the body, they become nearly as large as an egg, and blood flows from them to such an excess, that the strength of the patient is exhausted, and consumption frequently follows. From the small verrugas the flow of blood is greatest.. I knew an instance of a half-caste Indian who from a small verrugas below the ankle lost two pounds of blood.

I was not able to trace this disease to any other cause than that which the Indians assign to it. At all events, it is certain that travellers who abstain from drinking the water of the condemned springs, escape the verrugas; whilst those who only once taste such water are attacked by the disorder. It is the same with mules and horses. One of my mules which drank verruga water was attacked by a large tumour on the leg. The disease is notoriously prevalent in the village of Santa Olaya.

The medical treatment of the Verrugas by the Indians is quite empirical. They administer to the patient the infusion of a plant which they call *Huajra-Huajra;* that is, Horn-Horn. I never witnessed any convincing proof of its efficacy. Its operation appears to be merely sudorific. A preparation of white maize is also frequently given, and it has the effect of assisting the action of the skin. When the eruption of the verrugas is tardy, a few spoonfuls of wine are found to be of great service. Sudorific and purifying medicines, together with cutting out the large verrugas, and keeping the wounds for a time in a state of suppuration, has heretofore been found the best mode of treatment. An accurate chemical analysis of the water which the Indians declare to be *agua de verruga*, would be very desirable.

In the Quebrada of Canta, where the verrugas are less common than in that of Matucanas, another disease, called the Uta, is of very frequent occurrence. The Uta is a sort of cancer, and it is more fearful in its consequences than the verrugas. Probably in no country in the world do so many local diseases prevail as in Peru. Every valley has its own peculiar disease, which frequently does not extend beyond the boundary of a few square miles, and is quite unknown in neighbouring districts. The origin of these disorders is, doubtless, to be traced to certain mineral or vegetable influences as yet unknown. It is remarkable how unequally these baneful visitations affect the different races of the inhabitants. The Indians and the lighter classes of half-castes are most frequently attacked by the verrugas; the whites are less liable to the disease, whilst the negroes and people of the darker shades of mixed blood seldom suffer from it. The Indians and the Chinos are particularly liable to the uta. The caracha, of which I have already spoken, visits the negroes, the zamboes, and the mulattoes; the lighter-complexioned races being much less liable to it.

The Mareo or Veta, a peculiar sense of oppression in breathing at great heights.—In the heights of the Cordillera the effect of the diminished atmospheric pressure on the human frame shows itself in intolerable symptoms of weariness and an extreme difficulty of breathing. The natives call this malady the *puna* or the *soroche;* and the Spanish Creoles give it the names of *mareo* or *veta.* Ignorant of its real causes, they ascribe it to the exhalations of metals, especially antimony, which is extensively used in the mining operations. The first symptoms of the veta are usually felt at the elevation of 12,600 feet above the sea. These symptoms are vertigo, dimness of sight and hearing, pains in the head, and nausea. Blood flows from the eyes, nose, and lips. Fainting fits, spitting of blood, and other dangerous symptoms, usually attend severe attacks of veta. The sensations which accompany this malady somewhat resemble those of sea-sickness, and hence its Spanish name *mareo*. But sea-sickness is unaccompanied by the distressing difficulty of breathing experienced in the veta. This disorder sometimes proves fatal, and I once witnessed a case in which death was the result. Inhabitants of the coast and Europeans, who for the first time visit the lofty regions of the Cordillera, are usually attacked with this disorder. Persons in good health and of a spare habit speedily recover from it, but on plethoric and stout individuals its effects are frequently very severe. After an abode of some time in the mountainous regions, the constitution becomes inured to the rarefied atmosphere. I suffered only two attacks of the veta; but they were very severe. The first was on one of the level heights; and the second on the mountain of Antaichahua. The first time I ascended the Cordillera I did not experience the slightest illness, and I congratulated myself on having escaped the veta; but a year afterwards I had an attack of it, though only of a few hours' duration. The veta is felt with great severity in some districts of the Cordillera, whilst in others, where the altitude is greater, the disorder is scarcely perceptible. Thus it would seem that the malady is not caused by diminished atmospheric pressure, but is dependent on some unknown climatic circumstances. The districts in which the veta prevails with greatest intensity are, for the most part, rich in the production of metals, a circumstance which has given rise to the idea that it is caused by metallic exhalations.

I have already described the effect of the puna climate on beasts of burthen. Its influence on some of the domestic animals is no less severe than on the human race. To cats it is very fatal, and at the elevation of 13,000 feet above the sea those animals cannot live. Numerous trials have been made to rear them in the villages of the upper mountains, but without effect; for after a few days' abode in those regions, the animals die in frightful convulsions; but when in this state they do not attempt to bite. I had two good opportunities of observing the disease at Yauli. Cats attacked in this way are called by the natives *azorochados*, and antimony is alleged to be the cause of the distemper. Dogs are also liable to it, but it visits them less severely than cats, and with care they may be recovered.

The Surumpe, a peculiar disease of the eyes.—Another scourge of the traveller in the Cordillera, is the disease called the *surumpe*. It is a violent inflammation of the eyes, caused by the sudden reflection of the bright rays of the sun on the snow. By the rarefied air and the cutting wind, the eyes, being kept in a constant state of irritation, are thereby rendered very susceptible to the effects of the glaring light. In these regions the sky is often for a time completely overshadowed by snow clouds, and the greenish yellow of the plain is soon covered by a sheet of snow: then suddenly the sun's rays burst through the breaking clouds, and the eyes, unprepared for the dazzling glare, are almost blinded. A sharp burning pain is immediately felt, and it speedily increases to an intolerable degree. The eyes become violently inflamed, and the lids swell and bleed. The pain of the surumpe is the most intense that can be imagined, and frequently brings on delirium. The sensation resembles that which it may be imagined would be felt if cayenne pepper or gunpowder were rubbed into the eyes. Chronic inflammation, swelling of the eyelids, dimness of sight, and even total blindness are the frequent consequences of the surumpe. In the Cordillera Indians are often seen sitting by the road-side shrieking in agony, and unable to proceed on their way. They are more liable to the disease than the Creoles, who, when travelling in the mountains, protect their eyes by green spectacles and veils.

MATERIA MEDICA.

Coca, or Coqua Leaves, an exciting narcotic or intoxicating agent, *used by the Indians of Peru.*—Coca leaves are produced by a small bush—the *Erythroxylon coca* of Lambert—from six to eight feet high, resembling a black thorn in its numerous white blossoms and the lively bright green colour of its leaves. These leaves, after being gathered and carefully dried, are used by the Indians and other natives much as tobacco and spirits are by Europeans and Americans, or opium by Turks and Chinese, for the purpose of producing temporary excitement and apathy.

On the effects of their habitual use, the testimony of travellers is not quite unanimous. According to some, they produce invariably hurtful effects. Poeppig, who travelled in Peru between 1827 and 1832, represents these consequences as most deleterious to those who use them, and the habit, when once acquired, as quite irresistible. The Peruvian, according to Poeppig, is rather a melancholy being, prone to despondency and depression of spirits; but when he has recourse to the leaves of the coca, this mental gloom leaves him, his imagination becomes heated and his spirits elevated, and he enjoys the happiest visions amidst every appearance of external misery. When he once indulges in the use of this intoxicating drug, and feels how great gratification it affords, he is seldom capable of resisting the temptation to repeat the indulgence; and he then becomes a *coquero* or habitual coca-chewer—the most pitiable and wretched of all debauchees. The craving for the drug seems to come on periodically, in certain instances, exactly as the craving for spirituous liquors or opium attacks those who have given themselves up to the use of these poisons. The coquero then masticates his allowance of coca leaves, betakes himself to some desolate and secret retreat in a valley of the Andes, in a cave, or under a tree, and continues, it is said, for one, two, or more days in this state of half excitement, half reckless apathy, forgetful of the world, society, and its duties, and utterly indifferent to the dangers with which he is surrounded from storms, wild beasts, or the casualties of the forest. After the effects of this subside, he sometimes returns to his usual haunts, pale, haggard, and trembling. The coquero, who proceeds in the usual way, is generally soon rendered unfit for all the duties of society. He becomes listless, spiritless, and inactive, and generally falls a victim to premature death. In short, the man who is seized with the love of this drug, according to Dr Poeppig, is regarded as a ruined man.

By Dr Von Tschudi, the account given is a little different. He does not represent the use of the coca leaves as invariably hurtful. Their abuse only he condemns, and thinks that their moderate and temperate employment is and may be really advantageous to the Indian.

The coca is a shrub about six feet in height, with bright green leaves and white blossoms. The latter are succeeded by small scarlet berries. It is raised from the seed, in garden-beds called *almaxigas*. When the young shoots are one and a half or two feet high, they are removed to regularly laid out coca fields *(cocales)*, where they are planted at the distance of about three spans from each other. The coca requires humidity; therefore, during the first year or two after it is planted in the fields, maize is sown between the *matas*, or young shoots, to screen them from the too great influence of the sun. When the leaves are ripe, that is to say, when on being bent they crack or break off, the gathering commences. The leaves are stripped from the branches, a task usually performed by women, and it requires great care lest the tender leaves and young twigs should be injured. In some districts the Indians are so very careful in gathering the coca, that, instead of stripping off the leaves, they cut them from the stem by making an incision with their nails. The plant thus rendered leafless is soon again overgrown with verdant foliage. After being gathered, the leaves are spread out on coarse woollen cloths and dried in the sun. The colour of the leaves when dried is a pale green. The drying is an operation which likewise demands great care and attention; for if the leaves imbibe damp, they become dark coloured, and then they sell for a much lower price than when they are green. The dry coca is finely packed in woollen sacks and covered with sand. These sacks are of various sizes and colours, in different parts of the Montanas. In Huanuco they are grey or black, and when filled weigh from 75 to 80 pounds. In Vitoc they are grey and white, and contain 150 pounds. In Huanta and Anco they are small in size, and black or brown in colour, and contain merely one aroba. In the Montanas of Urubamba, Calca, and Paucartambo, the coca leaves are put into small baskets, called *cestos*, and covered with sand. Great care is also requisite in the carriage of the coca, for if damp be allowed to penetrate the sack, the leaves become hot, or, as the natives express it, *Se calientan*, and are thereby rendered useless.

The Indians masticate the coca. Each individual carries a leathern pouch, called the *huallqui*, or the *chuspa*, and a small flask gourd, called the *ishcupura*. The pouch contains a supply of coca leaves, and the gourd is filled with pulverised unslaked lime. Usually four times, but never less than three times a day, the Indian suspends his labour, for the purpose of masticating coca. This operation, (which is termed *chacchar acullicar*) is performed in the following manner: some of the coca leaves, the stalks having been carefully picked off, are masticated until they form a small ball, or as it is called an *acullico*. A thin slip of damp wood is then thrust into the *ishcupura*, or gourd, and when drawn out some portion of the powdered lime adheres to it. The *acullico*, or ball of masticated coca leaves is, whilst still lying in the mouth, punctured with this slip of wood, until the lime mixing with it gives it a proper relish, and the abundant flow of saliva thus excited is partly expectorated and partly swallowed. When the ball ceases to emit juice, it is thrown away, and a new one is formed by the mastication of a fresh mouthful of coca leaves. In Cerro de Pasco, and in places still further South, the Indians use, instead of unslaked lime, a preparation of the pungent ashes of the quinua (*Chenopodium Quinua*, L.). This preparation is called *Llucta* or *Llipta*. In using it a piece is broken off and masticated along with the *acullico*. In some of the Montana regions the Llucta is made from the ashes of the musa root. The application of the unslaked lime demands some precaution, for if it comes in direct contact with the lips and gums, it causes a very painful burning During a fatiguing ride across the level heights, where, owing to the cold wind, I experienced a difficulty of respiration, my Arriero recommended me to chew coca, assuring me that I would experience great relief from so doing. He lent me his *huallqui*, but owing to my awkward manner of using it, I cauterised my lips so severely that I did not venture on a second experiment.

The flavour of coca is not unpleasant. It is slightly bitter, aromatic, and similar to the worst kind of green tea. When mixed with the ashes of the musa root it is somewhat piquant, and more pleasant to European palates than it is without that addition. The smell of the fresh dried leaves in a mass is almost overpowering; but this smell entirely goes when they are packed in the sacks. All who masticate coca have a very bad breath, pale lips and gums, greenish and stumpy teeth, and an ugly black mark at the angles of the mouth. An inveterate *coquero*, or coca chewer, is known at the first glance. His unsteady gait, his yellow-coloured skin, his dim and sunken eyes encircled by a purple ring, his quivering lips and his general apathy, all bear evidence of the baneful effects of the coca juice when taken in excess. All the mountain Indians are addicted more or less to the practice of masticating coca. Each man consumes, on the average, between one ounce, and one ounce and a-half per day, and on festival days about double that quantity. The owners of mines and plantations allow their labourers to suspend their work three times a day for the *chacchar*, which usually occupies upwards of a quarter of an hour; and after that they smoke a paper cigar, which they allege heightens the zest of the coca mastication. He who indulges for a time in the use of coca finds it difficult, indeed almost impossible, to relinquish it. This fact I saw exemplified in the cases of several persons of high respectability in Lima, who are in the habit of retiring daily to a private apartment for the purpose of masticating coca. They could not do this openly, because among the refined class of Peruvians the chacchar is looked upon as a low and vulgar practice, befitting only to the labouring Indians. Yet, Europeans occasionally allow themselves to fall into this habit; and I knew two in Lima, the one an Italian and the other a Biscayan, who were confirmed coqueros in the strictest sense of the word. In Cerro de Pasco there are societies having even Englishmen for their members, which meet on certain evenings for the chacchar. In these places, instead of lime or ashes, sugar is served along with the coca leaves. A member of one of these clubs informed me that on the few first trials the sugar was found very agreeable, but that afterwards the palate required some more pungent ingredient.

The operation of the coca is similar to that of narcotics administered in small doses. Its effects may be compared to those produced by the thorn-apple rather than to those arising from opium. I have already noticed the consequences resulting from drinking the decoction of the datura. In the inveterate coquero similar symptoms are observable, but in a mitigated degree. I may mention one circumstance attending the use of coca, which appears hitherto to have escaped notice: it is, that after the mastication of a great quantity of coca the eye seems unable to bear light, and there is a marked dilatation of the pupil. I have also observed this peculiarity of the eye in one who had drunk a strong extract of the infusion of coca leaves. In the effects consequent on the use of opium and coca there is this distinction, that coca, when taken even in the utmost excess, never causes a total alienation of the mental powers or induces sleep; but, like opium, it excites the sensibility of the brain, and the repeated excitement, occasioned by its intemperate use after a series of years, wears out mental vigour and activity.

[This fact, as to the dilatation of the pupil, shows that in their effects on the animal economy, coca leaves must act in a manner something like henbane (*Hyoscyamus niger*), or deadly nightshade (*Atropa Belladonna*). Yet they differ from these plants in producing a degree of recklessness and indifference to evils, and great power of endurance under long-continued exertion and fatigue. It renders its votaries quite insensible to the feelings of hunger and thirst, or rather, it entirely extinguishes these feelings. Unlike opium, also, and other narcotics, it not only does not constipate, but is said to possess aperient qualities; and Dr Von Tschudi maintains that there is distinct proof that it is nutritious.]

It is a well-known fact, confirmed by long observation and experience, that the Indians who regularly masticate coca require but little food, and, nevertheless, go through excessive labour with apparent ease. They, therefore, ascribe the most extraordinary qualities to the coca, and even believe that it might be made entirely a substitute for food. Setting aside all extravagant and visionary notions on the subject, I am clearly of opinion that the moderate use of coca is not merely innoxious, but that it may be even very conducive to health. In support of this conclusion, I may refer to the numerous examples of longevity among Indians who, almost from the age of boyhood, have been in the habit of masticating coca three times a day, and who in the course of their lives have consumed no less than two thousand seven hundred pounds, yet, nevertheless, enjoy perfect health. The food of the Indians consists almost exclusively of vegetable substances, especially roasted maize and barley converted into flour by crushing, which they eat without the admixture of any other substance. The continued use of this farinaceous food occasions severe obstructions which the well-known aperient qualities of the coca counteract, and many serious diseases are thereby prevented. That the coca is in the highest degree nutritious, is a fact beyond dispute. The incredible fatigues endured by the Peruvian infantry, with very spare diet, but with the regular use of coca; the laborious toil of the Indian miner, kept up, under similar circumstances, throughout a long series of years; certainly afford sufficient ground for attributing to the coca leaves, not a quality of mere temporary stimulus, but a powerful nutritive principle. Of the great power of the Indians in enduring fatigue with no other sustenance than coca, I may here mention an example. A Cholo of Huari, named Hatun Huamang, was employed by me in very laborious digging. During the whole time he was in my service, viz., five days and nights, he never tasted any food, and took only two hours' sleep nightly. But at intervals of two and a half or three hours, he regularly masticated about half an ounce of coca leaves, and he kept an acullico continually in his mouth. I was constantly beside him, and therefore I had the opportunity of closely observing him. The work for which I engaged him being finished, he accompanied me on a two days' journey of twenty-three leagues across the level heights. Though on foot, he kept up with the pace of my mule, and halted only for the *chacchar*. On leaving me, he declared that he would willingly engage himself again for the same amount of work, and that he would go through it without food if I would but allow him a sufficient supply of coca. The village priest assured me that this man was sixty-two years of age, and that he had never known him to be ill in his life.

The Indians maintain that coca is the best preventive of that difficulty of respiration felt in the rapid ascents of the Cordillera and the Puna. Of this fact I was fully convinced by my own personal experience. I speak here, not of the mastication of the leaves, but of their decoction taken as a beverage. When I was in the Puna, at the height of 14,000 feet above the level of the sea, I drank, always before going out to hunt, a strong infusion of coca leaves. I could then, during the whole day, climb the heights and follow the swift-footed wild animals without experiencing any greater difficulty of breathing than I should have felt in similar rapid movements on the coast. Moreover, I did not suffer from the symptoms of cerebral excitement or uneasiness which other travellers have observed. The reason perhaps is, that I only drank this decoction in the cold Puna, where the nervous system is far less susceptible than in the climate of the forests. However, I always felt a sense of great satiety after taking the coca infusion and I did not feel a desire for my next meal until after the time at which I usually took it.

By the Peruvian Indians the coca plant is regarded as something sacred and mysterious, and it sustained an important part in the religion of the Incas. In all ceremonies, whether religious or warlike, it was introduced, for producing smoke at the great offerings, or as the sacrifice itself. During divine worship, the priests chewed coca leaves, and unless theywere supplied with them, it was believed that the favour of the gods could not be propitiated. It was also deemed necessary that the supplicator for divine grace should approach the priests with an *Acullico* in his mouth. It was believed that any business undertaken without the benediction of coca leaves could not prosper; and to the shrub itself worship was rendered. During an interval of more than 300 years, Christianity has not been able to subdue the deep-rooted idolatry; for everywhere we find traces of belief in the mysterious power of this plant. The excavators in the mines of Cerro de Pasco throw masticated coca on hard veins of metal, in the belief that it softens the ore, and renders it more easy to work. The origin of this custom is easily explained, when it is recollected that in the time of the Incas it was believed that the *Coyas*, or the deities of metals, rendered the mountains impenetrable, if they were not propitiated by the odour of coca. The Indians, even at the present time, put coca leaves into the mouths of dead persons, to secure to them a favourable reception on their entrance into another world; and when a Peruvian Indian on a journey falls in with a mummy, he, with timid reverence, presents to it some coca leaves as his pious offering.

Soon after the conquest of Peru, when the Spaniards treated the Indians and all their customs with contempt, coca became an object of aversion to the whites. The reverence rendered by the natives to the coca plant induced the Spaniards to believe that it possessed some demoniacal influence. The officers of the government and the clergy, therefore, endeavoured, by all possible means, to extirpate its use, and this is one cause, hitherto overlooked, of the hatred with which the Indians regarded the Spaniards. In the second council held at Lima, in 1567, coca was described "as a worthless object, fitted for the misuse and superstition of the Indians;" and a royal decree of October 18, 1569, expressly declares that the notions entertained by the natives that coca gives them strength, is an "illusion of the devil," *(una elusion del Demonio)*. The Peruvian mine owners were the first to discover the importance of the *chacchar* in assisting the Indians to go through their excessive labour, and they, together with the plantation owners, became the most earnest defenders of coca. The consequence was, that, in defiance of royal and ecclesiastical ordinances, its use increased rather than diminished. One of the warmest advocates of the plant was the Jesuit Don Antonio Julian, who, in a work entitled, "Perla de America," laments that coca is not introduced into Europe instead of tea and coffee. "It is," he observes, "melancholy to reflect that the poor of Europe cannot obtain this preservative against hunger and thirst; that our working people are not supported by this strengthening plant in their long-continued labours." In the year 1793, Dr Don Pedro Nolasco Crespo pointed out in a treatise the important advantages that would be derived from the use of the coca plant, if introduced into the European navies, and he expresses a wish that experiments of its utility in that way could be tried. Though it is not probable that Dr Crespo's wish will ever be realised, yet there is little doubt that the use of coca as a beverage on board ship would be attended with very beneficial results. It would afford a nutritious refreshment to seamen in the exercise of their laborious duties, and would greatly assist in counteracting the unwholesome effects of salt provisions. As a stimulant it would be far less injurious than ardent spirits, for which it might be substituted without fear of any of the evil consequences experienced by the *coqueros*. After a long and attentive observation of the effects of coca, I am fully convinced that its use, in moderation, is no way detrimental to health; and that without it the Peruvian Indian, with his spare diet, would be incapable of going through the labour which he now performs. The coca plant must be considered as a great blessing to Peru. It is an essential means of preserving the nationality of the Indians, and in some measure mitigating the melancholy fate of that once great race, which disease and excessive labour now threaten to destroy.

In former times the cultivation of coca in the Montana de Vitoc was very considerable. Upwards of 4000 arobas used to be annually forwarded to the market of Tarma. Now only fifty arobas are sent.

TOXICOLOGY.

Soon after Dr Simpson's discovery of the narcotic effects of chloroform was made known, A—— W——, a young man in the employment of Messrs Souter & Shepherd, wholesale druggists in Aberdeen, having occasionally to weigh a portion of chloroform, found himself so exhilarated under the effects of the vapour that escaped, that he was tempted to use it on his handkerchief as if it were eau de cologne, until a habit was formed which became a species of intoxication. His father, who was foreman in the establishment, on being made acquainted with the facts, endeavoured to dissuade his son from a practice so pernicious, but without effect; and it was soon observed that the nervous system of the young man was seriously disordered. On Tuesday, the 8th February, he was left in the warehouse with a younger boy, and having occasion to weigh an ounce of chloroform to order, could not resist the opportunity of inhaling the vapour. Having become very excited, his companion was alarmed; and knowing that he used to become violent when any attempt to reprove him in such a state was made, he was pleased to see the young man lay his head quietly down on his arms resting on the table. He was in this state when the principal clerk entered the warehouse; and on his father being called, it was found, on attempting to raise the head of the patient, that life was extinct. Medical assistance was speedily procured, and the jugular vein was opened, but no blood came.

This case should act as a warning against the use of chloroform by any person unless under the eye of a physician, and it should prevent chemists from allowing any inexperienced person to dispense the agent.

The deceased was in his 19th year.

It is observed that, as there is no coroner's inquest in Scotland, the benefits that might have resulted from a professional examination of the case are lost. The want of coroner's inquest need not have produced this effect; for in all cases of this kind, the law provides that the sheriff shall inquire into the cause of death and report to the lawyers of the crown.

LEGAL MEDICINE.

Fatal Application of Chloroform.—An inquest was held on Tuesday, the 1st of February 1848, by adjournment from Saturday, the 29th of January, at Winlaton, about five miles from Newcastle-upon-Tyne, before Mr Fevall, the coroner for Chester Ward, in the county of Durham, on view of the body of Hannah Greener, a girl 15 years of age, who died on Friday the 28th of January under the influence of chloroform, administered in order to allay sensibility while undergoing a painful surgical operation.

John Rayne, having been sworn said,—I am a blacksmith, and live at Winlaton. My sister is married to the deceased's father, whose name is John Greener. The deceased was an illegitimate child, whose mother died in childbirth. That was before her father was married to my sister. The deceased came to reside with John Greener and his wife about a year ago. She had previously lived with a person in Gateshead, and had been much thrust about. She suffered a great deal in her feet, and about four months ago she became an inmate of the infirmary at Newcastle-upon-Tyne, where she had one of her toe nails taken off. After she left the infirmary she returned to her father's, but her toes still continued bad. Mr Meggison, surgeon, of Whickham, was called in to attend her, and it was considered advisable to remove the nail from the great toe of her right foot. Mr Meggison and his assistant attended about 1 o'clock on Friday afternoon last to perform that operation. No one was present when the operation was performed but Mr Meggison, his assistant, and myself. The deceased was seated on a chair near the fire, and Mr Meggison having put something on a pocket handkerchief, held it to her mouth and nose, at the same time looking at his watch and feeling her pulse. She appeared to faint, when Mr Meggison told his assistant to take the nail off, and I held her leg to steady her foot. He took it off quickly. When he was taking it off, the deceased shook her leg, but did not speak. She moaned after the nail was off: on which Mr Meggison dashed water in her face, when her eye moved. He afterwards put some brandy into her mouth, and she rattled in her throat. He also bled her in the arm and neck, but very little blood flowed. She never recovered. When she had the nail taken off in the infirmary some stuff was given to her, she told me, which caused her not to feel any pain.

Mary Greener.—I am the wife of John Greener, the father of the deceased, who was 15 years of age. She was in the infirmary nine weeks, and I brought her out on the Monday before Christmas-day. Her father, on the Thursday night before the operation was performed at home, said she had better not have the stuff to make her insensible, as she complained of it having caused a heaviness in her head for some days after she had taken it in the infirmary. She said she would not have her nail taken off without it. We were all agreeable that Mr Meggison should perform the operation, and give her the stuff. She was in good health generally before she went into the infirmary; she fretted while there, but she never complained of ill-treatment. She grew thinner in the infirmary; and she also got thinner after she came out, but she has taken her food better lately than she did previously. She has complained of pain in the chest, and sometimes doubled herself up when in bed. She never did that before she went to the infirmary. She suffered much pain in her toes, and went to the infirmary to get one of her nails taken off. I think it was the pain being so great that prevented her thriving. The toe nails were growing into the flesh, which was much swollen. I poulticed her toes, and rotten flesh came off the one nail which was taken off.

Thomas Nathaniel Meggison, surgeon, having been duly cautioned, and asked if he wished to make any statement, said,—Hannah Greener died under my hands on Friday the 28th of January, while under the influence of chloroform, which I had given her for the purpose of producing insensibility during the operation of removing one of her toe nails. She was suffering from onychia. She never complained of pain in the chest to me. The pain in her toes might cause her to become thinner. I seated her in a chair, and put about a teaspoonful of chloroform into a tablecloth, and held it to her nose. After she had drawn her breath twice she pulled my hand down. I told her to draw her breath naturally, which she did, and in about half a minute I observed the muscles of the arm become rigid, and her breathing a little quickened, but not stertorous. I had my hand on her pulse, which was natural, until the muscles became rigid. It then appeared somewhat weaker—not altered in frequency. I then told Mr Lloyd, my assistant, to begin the operation, which he did, and took the nail off. When the semicircular incision was made she gave a struggle or jerk, which I thought was from the chloroform not having taken sufficient effect. I did not apply any more. Her eyes were closed, and I opened them, and they remained open. Her mouth was open, and her lips and face blanched. When I opened her eyes they were congested. I called for water when I saw her face blanched, and I dashed some of it in her face. It had no effect. I then gave her some brandy, a little of which she swallowed with difficulty. I then laid her down on the floor, and attempted to bleed her in the arm and jugular vein, but only obtained about a spoonful. She was dead, I believe, at the time I attempted to bleed her. The last time I felt her pulse was immediately previous to the blanched appearance coming on, and when she gave the jerk. The time would not be more than three minutes from her first inhaling the chloroform till her death. I purchased the chloroform from Messrs Dalgleish and Ismay of Newcastle. I have used chloroform from the same place before with good effect, and with no ill consequences I did not apply more than a drachm to the deceased, probably less. One person I gave it to required upwards of half-an-ounce, at four repetitions, before she became insensible, and she completely recovered afterwards. I have used it to a person who was very drunk, and had dislocated and fractured his ankle. Two spoonfuls were given in that case, and they had the desired effect, as he would not lie still without it. Mr Lloyd, who was present with me in the case of the deceased, is a regularly educated medical man, and he also has been in the habit of using chloroform.

Mr Lloyd.—I am the assistant to Mr Meggison, and I produce the bottle containing the remainder of the chloroform that was used for the deceased. The bottle originally contained an ounce. There was about a spoonful taken out. I have heard the statement of Mr Meggison and confirm it.

At this point the inquiry was adjourned on the Saturday; and, on the jury reassembling on Tuesday, the coroner stated that he had called in Sir John Fife and Dr Glover of Newcastle, to make a *post mortem* examination of the deceased. Sir John Fife was a medical practitioner, of extensive practice and long standing, on whose judgment great reliance might be placed; and Dr Glover had paid great attention to the nature of chloroform, and had written several articles on the subject. One of these articles appeared in the Medical Gazette of the 3d of December 1847, in which he referred to the recent application of chloroform in surgical cases by Dr Simpson of Edinburgh; and he stated that the effect of chloroform on the animal system had been known for some years; and he remarked that unless its effect on the human being was different from what it was on animals, as proved by experiments he had himself made, there was danger to be apprehended from its indiscriminate use. As the subject was of great importance to the medical profession and the public, he considered it proper to have the opinion of Dr Glover on this case, conceiving that from the attention he had paid to the subject it would be of great weight. The coroner then called,

John Greener, the father of the deceased, who, however, added little to the information of the previous witnesses. It was the deceased's own wish to have the chloroform applied, and all were willing that it should be used. Ether was used in the infirmary, and she told me they applied it of three different strengths before it took effect, and they tried if she were insensible to pain by pricking her with a pin. She felt no pain during the operation, but complained of her head afterwards.

Sir John Fife.—I am a surgeon in Newcastle-upon-Tyne. I made a *post mortem* examination of the body of Hannah Greener, the deceased, along with Dr Glover, about 3 o'clock on the afternoon of Saturday last. The body was that of a well-grown girl, of the age indicated. The legs were rather thin, calves not sufficiently fleshy, breast tolerably well developed, and, on the whole, thin. She was not devoid of fat, as appeared on proceeding to open the body. The body was perfectly free from spots or stains of any kind, except from the marks of phlebotomy, to which she had been subjected, and some slight livid stains about the neck. The toes showed the nature of the operations which had been performed. There was simply the ordinary degree of rigidity. The mouth was a little open. The eyes presented no appearance of congestion.

The lungs were not collapsed. One or two slight adhesions were encountered on separating them from the walls of the chest. The external appearance of both lungs, over the whole surface, but especially in the inferior portions, was that of organs in a very high state of congestion. They were mottled with patches of a deep purple, blueish, or scarlet hue. They were everywhere crepitant. Along the outer and anterior border of both lungs, particularly of the upper lobe of the left lung, were several emphysematous bubbles of small size. On cutting into the pulmonary tissue, it was found free from tubercles, unless some hard bodies about the roots of the bronchia, enlarged and partially indurated glands, could be so called. The pulmonary tissue was filled with bloody froth, which was also found in the interior of the bronchia mixed with mucus. There was no appearance of hepatization. On examining the larynx and trachea, the epiglottis was found reddened at the summit, of a vermilion hue. The mucous membrane of the larynx was redder than natural, mottled with vascular patches. The sinuses of the larynx contained a good deal of dark mucus. The œsophagus was healthy. The stomach was distended with food. Digestion had been going on at the time of death. The liver, kidneys, and spleen were more congested than usual. The heart contained dark fluid blood in both its cavities; very little in the left. Its structure, and that of the great vessels near it, were healthy. The brain, externally and internally, was more congested than usual, and the ventricles contained rather more than the usual quantity of serum.

In my opinion the cause of death was the congestion of the lungs, and that congestion I ascribe to the inhalation of chloroform. Of the power of chloroform to occasion such congestion no doubt can be entertained after the experiments of Mr Wakely and Dr Glover on animals. There does not seem to have been anything in the previous condition of the young woman to have prevented the surgeon from having recourse to chloroform, as a means of allaying pain, in one of the most painful operations of surgery. Such is my opinion of the effect of chloroform in lessening human suffering, and the small degree of danger attending its application in the cases that I have seen, that if I were myself required to undergo an operation, I would have no hesitation whatever in taking it. I have been using chloroform three or four times a-week ever since its efficacy in relieving pain was published, and I have never seen bad effects from it. In one instance, that of a woman who had to submit to the removal of a tumour, weighing about 3lb., and distributed over a surface about a foot square, Dr Glover and I administered about eight times the quantity of chloroform that was used in the case of the deceased. She recovered quickly, and was not worse after the operation than might have been expected from its formidable character. I have used it frequently in amputation, lithotomy, and a great many severe surgical operations, and never knew any bad consequence arise from it I attribute the fatal effect of the chloroform in the present instance to peculiarity in the constitution of the young woman. I am doubtful whether it arose from the state of the lungs, or the peculiar susceptibility of the nervous system. No effect could arise in the present instance from the ether she took on the 26th of October in the infirmary. I have no hesitation in affirming that the fatal issue of this case might have occurred in the hands of the most prudent and skilful surgeon that ever lived. It sometimes happens that a person will die from the shock of the operation, within a very few minutes after, and with nothing to show the cause—merely from the shock it gives the system. Those cases are rare. I think in such cases the same individuals would be influenced in a similar way with chloroform. The same susceptibility of nerve that would render the shock fatal would render the chloroform fatal. No human foresight, no human knowledge, no degree of science could have forewarned any man against the use of chloroform in this case. I have refused to apply it in some cases where the operation could be done in a moment, and was not attended with severe pain; but in every painful case I use it, and, unless it produced insensibility, I would go on repeating it to double the quantity used in this case, and more, if it were necessary. A case occurred lately in the infirmary where a great deal of blood flowed during the operation, which was a very tedious one; and chloroform was repeated during the operation without any bad result afterwards.

By a juror.—The immediate cause of death in the case of the deceased was congestion of the lungs. Congestion is the state of a body gorged with blood, like a blood-shot eye; it is sometimes gorged to such an extent as to burst the smaller vessels, and escape in dots or drops. I may state that chloroform was used lately in the case of a child 10 months old, when an operation, attended with no loss of blood, was performed, and no bad result followed.

Robert Mortimer Glover.—I am a surgeon, but practise as a physician, and was formerly lecturer at the Medical School in Newcastle-upon-Tyne. I have heard the evidence of Sir John Fife, and fully agree with it. Having heard the evidence of Mr Meggison, and the persons who were present when the operation was performed under which the deceased died, I am of opinion that the after-treatment to restore life was proper, under the circumstances, and in accordance with the views of the highest medical writers. I think chloroform is safer in larger operations, where there is great loss of blood, than in smaller operations. I have examined the chloroform, and find it pure. I consider chloroform more dangerous than ether; and that has been my opinion from the first. The effect of the chloroform will depend much on the constitution. There is a cure reported in the *Medical Journal* of last Friday, in which half the quantity of chloroform was used that was employed in this case, and produced very serious effects. Previous to the present case I should have no hesitation in using it. Chloroform is in use all over the kingdom.

The coroner then addressed the jury, briefly explaining the law of the case.

The jury then retired, and, after a few minutes' absence, returned with the following verdict:—"We are unanimously of opinion that the deceased, Hannah Greener, died from congestion of the lungs, from the effect of chloroform, and that no blame can be attached to Mr Meggison, surgeon, or to his assistant, Mr Lloyd."

The inquisition was then made out in the usual form, and the jury dismissed.

Dr Simpson has published on this case some critical observations, in which he maintains, that death was the effect, not of the inhalation of chloroform, but of suffocation by the brandy and water, which, he argues, must have, during the insensibility of the patient, gotten into the windpipe. On this point it is difficult, and not quite safe, for any one who was not present to give any opinion; and from expressing any opinion we therefore refrain.

Dr Murphy, in a recent tract on the Use of Chloroform in the Practice of Midwifery, expresses the opinion, that the cause of death is obscure; and while he allows that it is highly probable that, "if artificial respiration had been attempted when the vital powers were in a state of exhaustion, so as to sustain respiration and relieve the congestion in the lungs, the girl might have survived; that any fluid over the larynx must remove all chance of such a result; yet adds, that still there does not appear to be sufficient evidence to prove that this and not chloroform caused death;" and "that there was abundant evidence of the poisonous effect of chloroform."*

STATISTICS.

Report on the Mortality of Edinburgh and Leith for the year 1847. By James Stark, M. D., Fellow of the Royal College of Physicians.—The mortality of Edinburgh for the year 1847, amounted to 7026,—3514 being males, 3192 females, and 320 still-born. The mortality for the year 1845 was 3976, that for 1846 was 4887.

The mortality of Leith for the year 1847 amounted to 1031,—483 being males, 472 females, and 76 still-born. The mortality for the year 1845 was 544, that for 1846 was 868.

The mean temperature of the year was 47.42 Fahr., being 2 degrees colder than the mean temperature of 1846, but 1 degree and 2-10ths warmer than that of 1845. The range of temperature during the year amounted to 62 degrees, 83 being the highest and 21 the lowest degrees noted in the shade during that period. The quantity of rain which fell amounted to 21.82 inches, being 8.86 inches less than the fall during 1846, and 4.83 inches less than the fall of rain during 1845. West winds blew 135½ days, east 87½ days, south-west 41 days, north-east 39 days, north-west 26 days, south-east 14½ days, and other winds for shorter periods.

As the mortality of Edinburgh and Leith has been higher during the past year than it has ever previously been, it may not be uninteresting to show, from the facts which have already been published, that the same high mortality has very generally prevailed in the larger towns of Britain. The ratio of that increase, however, has varied in the different towns according to various circumstances which it is unnecessary here to mention.

The following table, then, exhibits the mortality of certain towns of Great Britain for the years 1845, 46, and 47, and the ratio of increase in 1847 above the mortality of 1845 and 1846. As the year 1845 was one of average mortality, the towns are arranged according to the comparative ratio of increase in 1847 above the mortality of that year.

Towns.	1845.	1846.	1847.	Increase per cent. of mortality during 1847 above that of 1845.	Increase per cent. of mortality during 1847 above that of 1846.
Glasgow*.........	7,509	10,854	18,081	140	65
Liverpool.........	7,371	9,713	17,271	120	77
Kilmarnock†.....	449	510	908	102	78
Leith..............	486	801	955	96	19
Edinburgh.......	3,688	4,594	6,706	81	45
Paisley...........	1,154	1,429	2,068	79	44
Carlisle	752	1,098	1,331	75	21
Manchester	6,022	7,810	9,540	58	22
Wolverhampton.	2,091	2,391	3,205	53	34
Birmingham.....	3,604	4,686	5,406	50	15
London	48,935	49,763	60,442	23	21

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* Chloroform in the Practice of Midwifery. By Edward W. Murphy, A. M., M. D., &c. London, 1848. 8vo. P. 28.

* The number of still-born for the year 1845 was not ascertained. The average number of still-born, viz. 750, was therefore deducted from the total burials for that year to reduce the numbers to the deaths alone.

† These numbers include the still-born, whose exact numbers could not be ascertained.

The most formidable class of diseases during the past year has been the zymotic (epidemic, endemic, and contagious) diseases. In 1846 the mortality from this class amounted in Edinburgh to 24 per cent. of the total mortality, but during the past year it increased to 39 per cent., and in Leith, to 29 per cent. of the total mortality. Typhus fever was the epidemic which caused this great increase, causing 22 per cent. of the total mortality in Edinburgh, but only 11 per cent. in Leith. Till October it seemed to be chiefly propagated by contagion, having been imported into the city by the Irish immigrants; but after this period it assumed all the characteristics of a true epidemic appearing over every part of the town and among all classes without its origin or continuance then, being attributable to other than epidemic agency. In Leith it broke out as an epidemic in September, and continued as such till the close of the year. The following is the percentage of epidemic diseases to the total mortality in Edinburgh (still-births excluded) for every month during the past year, and for the first two months of the present year.

1847.	January,......	20 per cent.	1847.	August, ...	40 per cent.
	February, ...	23 ...		September,...	44 ...
	March, ...	23 ...		October, ...	48 ...
	April,.........	30 ...		November,...	49 ...
	May, ...	33 ...		December, ...	51 ...
	June, ...	46 ...	1848.	January,......	47 ...
	July, ...	46 ...		February, ...	47 ...

The epidemic diseases are thus seen to have attained their maximum of intensity during December, when the mortality therefrom amounted to 16 per day, and exceeded the half of the total deaths.

It was formerly the belief of physicians, that when an epidemic prevailed, it swamped to a great extent all other diseases, attacking and cutting off by preference those who would have fallen victims to other diseases. The mortality of 1847 does not, however, bear out this view; but, on the other hand, shows that the deaths resulting from epidemics are superadded to the mortality from all other causes, and that the ordinary diseases continue to cut off the same proportion of cases as if no predominant epidemic prevailed. Nay more, last year's mortality even allows the inference to be drawn, that the epidemic constitution increased the fatality of the ordinary diseases, adding to them all the deaths from the prevailing epidemics. Thus, in 1846 there died from the ordinary diseases (excluding the epidemics) 3453 persons, but in 1847 the mortality from the same diseases amounted to 4027. The same fact was noticed in 1832 and 1837, when the epidemic cholera and the epidemic influenza raged here.

During the year, 1517 persons fell victims to Typhus fever in Edinburgh. Of these 76 only died during the first quarter of the year, 236 during the second quarter, 414 during the third quarter, and 791 during the last quarter. As the deaths during the first quarter did not exceed the average proportion dying per quarter in Edinburgh, it is apparent that typhus fever did not become prevalent here till after the first quarter, but, as was pointed out in the monthly reports, began to extend its ravages in April, and continued, with few exceptions, steadily increasing in severity and fatality till the close of the year. In Leith, on the other hand, typhus fever did not break out as an epidemic till the last month of the third quarter of the year. Only 17 died from it during the first quarter, 11 during the second quarter, 20 during the third quarter, while during the last quarter the deaths increased to 60. The probable causes of this peculiarity will be alluded to afterwards.

This epidemic occurred in every varying degree of mildness from the slight febricula to the worst type of spotted or petechial typhus. The mortality of the milder forms varied from one death in 20, to one death out of every 25 cases; but as the fatality of the severer forms and of the typhoid cases rose so high as one out of every 3 or 4, the general mortality of the disease here up to the middle of November was ascertained to have been very nearly one out of every 8 attacked with the disease. From the middle of November, when influenza broke out, the fatality of all the fever cases increased, so that the mortality rose to a fraction more than one death out of every five cases, and continued at this high rate till the close of the year. Taking the average of one death for every 8 cases of fever, gives 12,136 as the number of fever cases in Edinburgh during the past year, or about one case of fever for every 12 inhabitants of the city, according to the census of the population for 1841. In fact, during the year there have died from fever alone 376 persons more than died during 1846 from the whole numerous class of zymotic diseases, including typhus fever, measles, small-pox, hooping-cough, scarlet fever, diarrhœa, cholera, &c.

One circumstance relative to the typhus fever of Edinburgh of the past year deserves notice, that is, the great disproportion between the sexes. Thus, of the 1517 deaths from this cause, 924 were males, but only 593 females; in other words, the males were upwards of a third more numerous than the females. As the disease first appeared among the immigrant Irish who flocked to Edinburgh to avoid the famine and pestilence in Ireland, and the greater proportion of these were males, the disproportion between the sexes may be accounted for in this way. During the latter portion of the year, again, when the mortality rose higher, and the cases were of a more severe type, the disbanding of the numerous labourers on the railways, and the flocking of these to town, once more furnished a numerical disproportion of male cases. In Leith, on the other hand, which was not subjected to the inroads of the Irish, the number of male and female deaths from fever was exactly equal.

Typhus fever has this unfortunate peculiarity, that its chief victims are those in the prime of life. Thus during the past year, of the 1517 deaths from this cause, only 240 were below 15 years of age, and 107 above 60, while 120 were between 15 and 20 years of age, 327 between 20 and 30 years, 267 between 30 and 40 years, 266 between 40 and 50 years, 163 between 50 and 60 years, and 26 adults whose exact ages could not be ascertained. The youngest child recorded as dying of typhus fever was 24 days old, the oldest person was in his 90th year.

The next most fatal zymotic disease in Edinburgh during the past year was Hooping-cough, which cut off 279 persons (amounting to 4 per cent. of the total deaths), 131 being males, and 148 females. There was thus an increase of 28 deaths on this disease during 1847 over the mortality from this cause in 1846. In Leith this disease was somewhat more prevalent than in Edinburgh, and the deaths therefrom proportionally greater by about a half per cent.

217 deaths from Measles were registered in Edinburgh during 1847, being 34 more than during the previous year; being in the proportion of 30 out of the 1000 deaths. Of these 111 were males, and 106 females. In Leith this disease was much less prevalent, the deaths being only in the proportion of 16 to the 1000 deaths.

Small-pox has been unusually prevalent, but especially in Leith, during the past year. In Edinburgh the deaths therefrom were in the portion of 24 out of the 1000 deaths, but in Leith they were in the proportion of 67 out of the 1000 deaths, or nearly three times as numerous. It is melancholy to observe that, of the 227 deaths from this disease in Edinburgh and Leith, 218 were registered as having occurred in persons not protected by vaccination. Of the other 9 cases 7 were thought to have been previously vaccinated, but nothing was known of the remaining 2 cases. On looking over the recorded deaths from small-pox in Edinburgh, from the introduction of vaccination in 1799 to the present day, it is impossible to fail remarking that the number annually dying from small-pox is on the increase. During the first decade of the present century (1800 to 1809), the deaths from small-pox averaged 45 annually. During the second decade they numbered 27 annually. During the third decade, 42 annually. During the fourth decade, 83 annually. While, during the present decade, the deaths have averaged no less than 110 annually. In my "Inquiry into the probable cause of the continued prevalence and fatality of Small-pox," published in 1845, it was shown that this was owing to the neglect of vaccination, and every succeeding year furnishes additional proofs of the same fact. In England, with all the assistance which is afforded by the Vaccination Act, we learn from the Twelfth Annual Report of the Poor Law Commissioners, that "the number of children vaccinated under one year of age is only 32 per cent. upon the number of births;" and if to this number we add 20 per cent. for the children of the upper classes vaccinated by private practitioners, which is a very large proportion, there is still left nearly a half of all who are born unprotected in England by vaccination. Since vaccination has begun to be so much neglected we have had small-pox as an epidemic among us every second or third year, as for instance in 1837, 1840, 1842, 1845, and 1847;—and unless some active means are taken to enforce vaccination on every child shortly after birth, we may soon see this most loathsome disease as prevalent and fatal among the lower classes as it was before the introduction of vaccination.

Scarlet fever towards the end of the year began to increase, but only 23 deaths from that cause were registered in Edinburgh and Leith during the year.

Croup was both prevalent and fatal during the last quarter of the year, more especially after the outbreak of influenza; for while 12 only died from this cause in Edinburgh during the third quarter, the mortality of croup rose to 35 during the last quarter.

The mortality of Influenza was almost entirely limited to the month of December. Leith suffered less severely than Edinburgh; the deaths there being in the small proportion of 15 per 1000 of the total deaths, or 5.4 out of every 10,000 of the population, while the deaths in Edinburgh were in the proportion of 18 per 1000 of the total deaths, or 8.9 out of every 10,000 of the population. This fact, therefore, shows the influence of exposed site on the prevalence and fatality of the disease, the low-lying comparatively sheltered situation of Leith rendering the influenza milder and less prevalent than in high-lying exposed Edinburgh.

As much unnecessary alarm is created every succeeding year by the report that epidemic or Asiatic Cholera has appeared among us, a few remarks on this disease may properly be added. Ever since the epidemic cholera was here in 1832, a few cases have annually occurred, which, in so far as mere symptoms were concerned, could not be distinguished from that disease. Three or four deaths from such suspicious cases occurred in Edinburgh and Leith* both during last and the previous year; but there was nothing to warrant the conclusion that such cases were epidemic cholera, seeing that, *unless the disease assumes the form of an epidemic*, it is quite impossible, from the mere symptoms, to assert that the cases are other than severe ones of British cholera. There is no one symptom or series of symptoms diagnostic of the epidemic cholera, but the circumstance of its assuming the epidemic type; and so long as the suspicious cases which occur do not assume this type, we can never venture to assert that that formidable epidemic is here. Some writers have ventured to assert that certain symptoms are diagnostic of the epidemic cholera; but those who really paid any attention to the varying symptoms of the epidemic of 1832 will be found to acknowledge that during that year, death itself ensued in several cases in which not one of the so-called diagnostic symptoms were remarked. This fact I myself witnessed. Unless the cholera, therefore, appears as an epidemic, we have nothing to fear from the occurrence of a few suspicious cases, even though we go the length of admitting them to be sporadic cases of the Asiatic disease.

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* Three deaths from cholera occurred in Leith during 1847; but as the particulars of the cases were not procured till after the tables were made up, they were registered under the head of causes of death not specified.

Of the other diseases, the mortality during 1847 was, with a few exceptions, almost the same as during the previous year;—the rise being almost entirely confined to diseases of the respiratory and digestive organs, to childbirth, and to old age.

Diseases of the Brain and nervous system were at their minimum during the first quarter of the year, only 117 deaths occurring during that period. During the last quarter, the deaths from this cause rose to 149. The chief rise on this class of diseases during the last quarter was on hydrocephalus (water in the brain), when 53 died from that disease, only 32 having died from that cause during the first quarter. Apoplexy and paralysis were least fatal during the third quarter of the year—the deaths respectively amounting to 19 and 18. During the last quarter, apoplexy cut off 27 and paralysis 29 persons.

Diseases of the Respiratory organs (not including influenza, hooping-cough, or croup, which are zymotic diseases) were most fatal during the first quarter of the year, and least fatal during the third quarter. Thus, during the first quarter, the deaths from this cause amounted to 415; during the second quarter to 313; during the third quarter to 267; and during the fourth quarter to 390,—thus very distinctly showing the influence of temperature on the fatality of this class of diseases. Pneumonia was unusually prevalent during the first quarter of the year—90 deaths being registered during that period. This disease declined in frequency till the end of the third quarter, at which period only 34 deaths were registered. The severe weather, however, during November and December, again raised the mortality of pneumonia to 74 for the last quarter. Consumption was most fatal during the second quarter of the year,—indeed, it has long been noticed that the spring months are most fatal to those labouring under that disease. Bronchitis in its fatality showed its close dependence on temperature. Thus, during the first quarter, it caused 53 deaths; during the second quarter 12 deaths; during the third quarter only 7 deaths; but during the fourth quarter 41 deaths. If to this last number the deaths from the epidemic bronchitis (influenza) be added, the total deaths from bronchitis during the last quarter would amount to 161.

Notwithstanding the greater prevalence of bowel complaints in 1846, the mortality from inflammatory affections of the organs of digestion during 1847 greatly exceeded that of 1846. Thus, in Edinburgh, the deaths from diseases of the digestive organs amounted to 493 in 1846, but increased to 598 during 1847. The whole increase was limited to one class of cases, viz., inflammation of the bowels, the deaths from which in 1846 amounted to 95 only, but in 1847 had increased to 193. The mortality from this cause was greatest during the last quarter, when the alternations of temperature and the epidemic constitution of the air were greatest. The same fact was observed at Leith. During 1846, the deaths from inflammation of the bowels amounted to 10 only, but during 1847 they increased to 35.

The deaths from child-birth during 1846 amounted in Edinburgh to 43 only, but during the past year they increased to 74. This increase occurred chiefly during the last quarter of the year when many cases of puerperal fever occurred. In Leith, the increase was much less considerable, but the proportion of deaths from childbirth to the total mortality was greater than in Edinburgh.

As the mortality during the past year has been unexampled within any period of which distinct records are preserved, it may not be uninteresting to notice the various fluctuations it has undergone during the several quarters of 1847, in order that the knowledge of this may enable us to arrive at some more determinate conclusions as to its causes.

During the first quarter of 1847, the mortality in Edinburgh, excluding the still-born, amounted to 1403, being an average of 467 monthly, or a fraction more than 15 deaths daily. During the second quarter of the year, the deaths amounted to 1526, and during the third quarter to 1527,—giving an average of 509 deaths per month, or nearly 17 per day. During the fourth quarter, the deaths amounted to 2251, yielding an average of 750 per month, or 25 per day. With reference, however, to the last quarter, it is necessary to notice that the mortality of October was the same as that of the six previous months, viz. 17 deaths daily; that of November increased to 728, or averaged 24 deaths daily; while the mortality of December, amounting to 1001, averaged no less than 32 deaths daily.

As a means of comparison with the above, it may be stated that the average mortality for the seven previous years, viz. from the commencement of 1840, has been 331 deaths per month, or 11 deaths daily. But as the first and fourth quarters usually exhibit an increased number of deaths, the average gives 10 deaths daily for the second and third quarters, and 12 deaths daily for the first and fourth quarters of the year.

From the above statement three conclusions may be drawn. *First*, That some cause must have produced the increased mortality during the first quarter, which was not in operation during the previous year. *Secondly*, That in the beginning of the second quarter, some additional source of sickness and mortality must have come into operation, which, instead of allowing the mortality to fall below that of the first quarter, increased the deaths by two daily, and continued uninterruptedly and steadily producing the same effect till November. *Thirdly*, That during November and December, some other and more powerful cause of sickness and death must have come into operation, which raised the deaths from 17 to 32 per day. The probable causes of these variations in the mortality will now be endeavoured to be pointed out.

The failure of the potato crop in the autumn of 1846, the deficiency of the corn crop, and the prevalence of the epizootic disease among cattle, raised the price of all kinds of provisions to a most exorbitant height. The consequence was, that most of the poorer classes who in ordinary years subsist chiefly on oatmeal porridge, potatoes, and milk, were obliged to substitute unazotized substances, such as treacle or beer, for the highly azotized principle milk, and use other substitutes for the potatoes. As oatmeal, however, in proportion to its real value, rose much higher in price than wheaten flour, porridge also was in many cases given up, and bread, with tea or coffee, substituted. Very little animal food was used by the lower classes, and milk was too scarce to be obtained by them. The consequence was, that scurvy in all its varied forms made its appearance, and appeared to render all more liable to be affected by the atmospheric vicissitudes and external agencies than if they had remained in vigorous health. This scorbutic tendency was not confined to the poor alone—though it was only among them that the severer forms of scurvy were remarked—but was observable to a small extent in all classes, among many of whom it could not be traced to any deficiency of proper nutritious food, unless it arose from the simple want of the potato, but appeared alone attributable to epidemic influence. Hence it happened that during the first quarter of the year, the increase in the number of deaths above the average mortality in Edinburgh was almost solely confined to persons labouring under diseases of the respiratory organs, and to those registered under the head of old age,—two classes much influenced by atmospheric agencies. During this quarter, no epidemic disease existed which could account for the unusual mortality, for scurvy, though very general, caused very few deaths; and even typhus fever itself was in Edinburgh rather below than above the average.

During the first month of the second quarter (April), typhus fever broke out and rapidly extended its ravages, not so much, however, among the resident population, as among the immigrant Irish, who flocked to this city in immense numbers to avoid the pestilence and famine in Ireland, and the typhus fever, which they had been the chief agents in importing into Glasgow. Many of these arrived labouring under the disease, others brought the seeds of the disease with them, and were seized therewith very shortly after their arrival; and so infectious was it, that wherever they lodged they communicated the disease to almost all coming in contact with them. From the period of these arrivals, and the consequent prevalence of fever, the general mortality rose from 15 to 17 deaths daily, and so long as these causes were kept up, continued at the same unvarying height till the middle of November, when another cause came into operation. Now it is not a little remarkable that if the deaths from typhus fever during the above period (viz. from April to October included) were subtracted from the general mortality, the average number of deaths per day would be only one daily above the daily average of the past seven years, viz. only 12 daily,—thus showing that nearly the whole increase in the number of deaths during the above seven months was attributable to the prevalence of the typhus fever which had been imported into the city.

The word imported has been designedly used to express the leading characteristic of the typhus fever which has been raging here for so many months. Unless a disease assumes the form of an epidemic, that is, breaks out about the same time among various classes of the population, and in different localities, without its rise being attributable to imported contagion, it does not seem reasonable to term that disease an epidemic. Now, the typhus fever in Edinburgh, from its first appearance in the beginning of April, till October, except in a few isolated cases, owed its origin and propagation almost entirely to imported contagion. After this it assumed the true epidemic type. It hung about, indeed, was for a long period almost wholly confined to the lodgings and low hovels frequented by the immigrant Irish, and seized all coming in contact with them; whereas many closes and places where in epidemic years typhus fever rages, wholly escaped during the above period, apparently because they were not frequented by the Irish. Indeed, so contagious was the fever which these immigrants carried with them, that numerous instances occurred in which it was ascertained that the same family communicated the disease to every lodging-house they entered, though at the moment none of the family were labouring under the disease. Even cleansings, fumigations, and whitewashings, which invariably alleviate or check typhus fever when epidemic, and aggravated by local causes, appeared to have little or no effect in checking the disease or moderating its virulence; even within a few days after these poor people were allowed to return to their cleansed dwellings, fever among them was as bad as before, seeing they brought the infection along with them.

In proof of this view may be instanced the fact, that so late as June, when all the wards of the Royal Infirmary and several temporary hospitals were filled with fever patients, and the fever had been raging for nearly three months, it was ascertained that by far the greater proportion of fever patients were Irish. Thus, of 473 fever patients in that institution on the 10th of June, 379 were natives of Ireland, only 87 of Scotland, and 7 of England. Again, the whole excess of mortality in Edinburgh during the greater portion of the second and third quarters was confined to the very lowest classes of the resident, and to the immigrant population, and in particular to that portion which is interred at the public expense. Hence, the city burying-ground, which alone received the bodies of paupers for burial, was almost the only one which exhibited an increase in the number of its burials during that period. Instead of interring from 85 to 90 bodies per month, the number increased to upwards of 200. The same fact was evidenced from what occurred at Leith—a town in the immediate vicinity of Edinburgh, but not subject to the inroads of the Irish who flooded Edinburgh. When typhus fever occurs as an epidemic, Leith, in proportion to its population, suffers somewhat more severely than Edinburgh; but even in the month of August not one death from typhus fever was registered as occurring in Leith, while the mortality from fever in Edinburgh during that month amounted to 131.

Another argument in favour of the view that typhus fever in Edinburgh was chiefly an imported disease was the fact, that the great majority of cases and of deaths were of the male sex. Thus, of 1517 deaths from fever, 924 were males and 593 females. Had the fever, however, depended principally either on epidemic, that is, atmospheric, or on endemic, that is, local causes, a much larger number of females would have fallen victims to it, at least they would have equalled the males in number. This is evident from the circumstance that females are more numerous in our population than males; so that, if the fever originated in epidemic causes, the females, from being more exposed to the aggravating local causes, would at least have been affected in equal numbers with the males. Many facts might be adduced to prove this point. In Leith, where the disease was clearly epidemic, the deaths among the males and females during the past year were exactly equal in number. In Glasgow during the year 1843, when typhus fever prevailed in a truly epidemic form, 671 males died, but no less than 726 females. In Dundee during the years 1839, 40, and 41, there died of fever 178 males and 186 females. And lastly, if we take all England, we shall find from the Registrar-General's Reports, that the very same position is proved, viz. that during epidemics of fever, the deaths among the females from that cause somewhat exceed the males in number. Thus in 1838, over all England, there died from fever 7643 males, but 8023 females. In 1839, the deaths from fever were 9282 males, but 9493 females; in 1842 the male deaths numbered 7056, the female deaths 7971, &c.

These two causes, then—the Scurvy during the first quarter, and the Typhus fever during the second and third quarters—contributed to keep up the mortality till the middle of November, when a new cause came into operation, viz. the Epidemic Bronchitis or Influenza. This disease, unlike the typhus fever, was clearly dependent on, and was traceable to, atmospheric agencies. During the last quarter, it twice suddenly attacked great masses of the population, *first*, on the 18th of November, and *again* on the 28th of the same month. In both cases the attacks were made during the same kind of weather, viz. sudden intense frost, attended with a thick, damp, wetting fog, immediately following very mild soft weather; and the number of persons laid up on each of these days was so great, and the attacks so general, that it was quite impossible to avoid noticing the coincidence. It is not meant to be stated that these meteorological phenomena were the sole causes of the influenza, though it is possible they might be, but they were the only apparent and tangible ones; there was probably also connected with these some unknown influence which caused these damp fogs and sudden depression of temperature to produce the epidemic catarrh. It is instructive to note, that from the 18th of November, the day when influenza first broke out, the mortality rose till it attained its maximum on the 30th of that month, two days after its second general attack, during which day no fewer than 61 deaths were registered, the usual winter average being only 12 deaths daily. Up to this period, only 15 deaths were ascribed to influenza, so that this great increase of mortality was not caused by that disease, but by the atmospheric agencies acting prejudicially on all other diseases, aggravating their symptoms, and rendering them more than usually fatal. From the 30th of November, the general mortality began slowly but steadily to decline. For the first four days of December, the deaths averaged 48 daily; during the next seven days the average was 37½ deaths daily; during the second seven days the average was 35 deaths daily; while, for the last two weeks of the month, the average fell to 21 deaths daily. These facts, then, clearly show that it was one general cause which gave rise to the outbreak of influenza, and to the great increase in the general mortality; and though we may be unable to point out the unknown causes of these, it is not a little satisfactory to be able to trace both to such apparent and intelligible atmospheric changes as those which attended the outbreaks of the epidemic catarrh.

During the continuance of the influenza, much trouble was taken to ascertain whether a high and exposed situation, or a low sanitary condition of a district rendered those inhabiting such localities more liable to the disease. *First*, It was distinctly ascertained that the inhabitants of exposed situations were much more severely and more generally affected than those living in low sheltered sites. Thus the inmates of George Heriot's Hospital, on a high exposed site, suffered severely, a full half being in bed on the 29th November; while in George Watson's Hospital, immediately adjoining, but on a low sheltered site, very few indeed were affected, and none severely. In the Merchant Maiden Hospital, on a low lying but exposed site, of 86 girls 50 were confined to bed with influenza on the 29th November; whereas, in the Trades' Maiden Hospital, also on a low but very sheltered site, not an inmate was affected by the disease. *Secondly*, The low sanitary condition of a district was not found to render those inhabiting it more liable to be affected with the disease. Of children living in the Cowgate, Grassmarket, and other low-lying close and dirty situations, and attending school, only 35 per cent. were absent from school from influenza on the 29th November, and those attending school appeared quite healthy. Of the children of the upper classes, in the exposed New Town, living in well-aired cleanly houses, and attending school, 62 per cent, were confined with influenza, and many attending were so ill they ought to have been kept at home. Many similar facts might be adduced, proving the same point—viz. that the severity of the attacks of influenza was regulated by the exposure of the situation, and not by the sanitary condition of the district.

These three great causes, then, 1*st*, the scorbutic tendency; 2*d*, the typhus fever; and, 3*d*, the influenza and its atmospheric causes, appear to have been the chief agents in increasing the mortality of the past year.

Having thus noticed the three special causes of the high mortality of the past year, some allusion may be made to those general causes which have a marked influence on all of them. That the weather has a marked influence on the mortality there can be no doubt; but as this influence has been so frequently pointed out in the monthly reports, it need not be enlarged on here. But there is another cause which experience and observation has also shown to have a marked effect on the general mortality, viz. the failure of the crops and the consequent high price of provisions. M. Messance, in his interesting work entitled "Recherches sur la Population," &c., published in 1764, shewed the close connection between the number of deaths in most of the towns and provinces of France, and the price of grain. His work gives the statistics of mortality and the prices of grain from 1674 to 1764, a period of ninety years. M. Melier took up the same subject in 1843, and traced the connection of the same phenomena from 1764 up to that date. These two valuable works thus form a body of evidence of the most conclusive kind as to the close connection of high mortality with scarcity and high price of provisions. One example of this influence may be quoted from these works. The four most unhealthy years which occurred between 1744 and 1763 were selected and compared with the four most healthy years within the same period, when it was found that in Paris the average annual mortality amounted to 20,895 during the unhealthy years, but only to 16,859 during the healthy years, or 4036 fewer deaths during each healthy year. The average price of wheat during the four unhealthy years was 19 livres 1 sous (about 15s. 10½d.) the setier (a little more than 4 bushels); but only 14 livres 8 sous (about 12s. 5d.) the setier, during the four healthy years. During the present century, however, it is shown that the influence of years of scarcity on the general mortality has greatly diminished; and M. Melier accounts very rationally for this fact. During the last century agriculture was comparatively in its infancy, and a failure of the crops deprived thousands of the means of living. Of late years, however, not only was a much larger proportion of grain raised as compared with the number of the population, but the introduction of the potato, he thinks, has been the principal means of saving the people from the effects of years of scarcity, seeing that that root was every year more largely cultivated, and often succeeded best in France in years when the corn crops failed. The failure of the potato crop, however, during the autumn of 1846, seeing that it constituted such a large proportion of the food of all, but especially of the lower classes, proved a very serious matter. This failure of the potato crop, the scantiness of the grain crops, and the consequent high price of all kinds of provisions, have, during the past year, had precisely the same effect in increasing the general mortality as the same causes have been observed to have had in all former periods of history, but especially before the introduction of the potato. It is not necessary, however, to consider the increased mortality to depend on deficient quantity or unwholesome quality of the food: it is much more philosophical and consonant with observations to believe that the failure of the crops and the increased mortality are the offspring of a common cause. Noah Webster in his interesting "History of Epidemic and Pestilential Diseases," has collected many striking proofs of the connection of increased mortality (pestilential periods) with many phenomena of the natural world, such as bad seasons, failure of the crops, disease among cattle, earthquakes, &c.; and those who wish to study the matter more deeply, will find much food for serious thought in the facts which he has collected, and the conclusions he has drawn from them.

The following Tables exhibit abstracts of the Mortality Tables of Edinburgh and Leith for the year 1847, classified according to Ages and Diseases. Appended to these is a Table giving a summary of the meteorology alongside of the mortality for the year.

I.—EDINBURGH AND LEITH TABLE OF AGES FOR 1847.

AGE.	EDINBURGH.			LEITH.		
	Males	Female	Total.	Males.	Female	Total.
1 year and under,	545	449	994	96	82	178
2 .	313	295	608	58	53	111
5	261	233	494	36	43	79
10 .	126	111	237	14	15	29
15	58	48	106	5	7	12
20 .	155	110	265	21	14	35
30	420	311	731	32	33	65
40 .	358	298	656	48	38	86
50	395	316	711	34	37	71
60 .	296	298	594	46	35	81
70	284	332	616	46	51	97
80 .	198	258	456	33	43	76
90	68	99	167	10	15	25
100 .	7	12	19	0	0	0
111	0	1	1	0	0	0
Not stated, .	30	21	51	4	6	10
Total deaths,	3514	3192	6706	483	472	955
Still-born, .	192	128	320	46	30	76
Total, .	3706	3320	7026	529	502	1031

II.—EDINBURGH AND LEITH CLASSIFIED TABLE OF DISEASES FOR 1847

Class.	Diseases.	Edin.	Leith.
I.	Zymotic dis. (epidemic, endemic, and contagious)	2679	285
II.	Diseases of uncertain or variable seat, .	428	64
III.	Diseases of brain and nervous system, .	516	84
IV.	Diseases of respiratory organs, . .	1385	212
V.	Diseases of heart and blood-vessels, .	114	14
VI.	Dis. of stomach, liver, and other organs of digest.	598	106
VII.	Diseases of kidneys and urinary organs, .	39	2
VIII.	Child-birth, and diseases of organs of generation,	87	16
IX	Rheumatism, diseases of bones, joints, &c. .	27	7
X.	Diseases of the integumentary system,	8	2
XI.	Old age, ...	617	115
XII.	Intemperance, violent deaths, privation, suicides.	141	34
	Disease not specified, . . .	67	14
	Total deaths, . .	6706	955
XIII.	Still-born, ...	320	76
	Total, . .	7026	1031

CLASSIFIED TABLE OF DISEASES FOR 1847.

Class.	Disease.	Edinburgh.	Leith.
I.	Small-pox, (vaccinated) .	6	3
	Do. (not vaccinated) .	157	61
	Measles, ...	217	16
	Scarlet fever, . .	20	3
	Hooping-cough, . . .	279	42
	Croup, ...	89	13
	Thrush, . . .	20	...
	Diarrhœa, ...	118	9
	Dysentery, . . .	32	1
	Cholera, ...	11	...
	Influenza, . . .	125	15
	Remittent fever, . .	22	3
	Typhus fever, . . .	1517	108
	Erysipelas, ...	59	11
	Syphilis, . . .	7	. .
II.	Hæmorrhage, . .	19	1
	Anæmia, . . .	3	2
	Dropsy, ...	154	26
	Abscess, . . .	6	.
	Mortification, . .	8	1
	Purpura and scurvy, .	7	...
	Scrofula, ...	4	1
	Cancer, . . .	26	7
	Tumour, ...	4	...
	Atrophy, . . .	3	1
	Debility, ...	178	13
	Malformation . . .	1	1
	Sudden death, . .	15	11
III.	Cephalitis, . . .	29	7
	Hydrocephalus, . .	165	29
	Apoplexy, . . .	84	13
	Paralysis, ...	101	11
	Convulsions, . . .	39	8

Class.	Disease.	Edinburgh.	Leith.
	Tetanus,	6	...
	Chorea,	2	...
	Epilepsy	12	1
	Insanity,	36	11
	Delirium tremens,	24	2
	Disease of brain,	17	2
	Tic douloureux,	1	...
IV.	Larygnitis,	12	...
	Quinsey,	15	2
	Bronchitis,	113	27
	Pleurisy,	21	1
	Pneumonia,	246	71
	Hydrothorax,	34	3
	Asthma,	118	14
	Consumption,	799	93
	Lung disease,	27	...
V.	Pericarditis,	4	...
	Aneurism,	3	...
	Heart disease,	107	14
VI.	Teething,	118	18
	Gastritis,	3	1
	Enteritis,	190	34
	Peritonitis,	13	3
	Tabes mesenterica,	161	34
	Worms,	1	...
	Ascites,	1	1
	Ulceration of intestines,	17	2
	Hernia,	11	1
	Colic or ileus,	4	...
	Intussusception,	4	...
	Stricture of intestines,	3	...
	Stomach disease,	15	5
	Hepatitis,	2	...
	Jaundice	11	1
	Liver disease,	43	6
	Spleen disease,	1	...
VII.	Nephritis,	3	...
	Ischuria,	1	...
	Diabetes,	9	...
	Stone,	4	...
	Stricture of urethra,	5	...
	Kidney disease,	17	2
VIII.	Child-birth,	74	13
	Paramenia,	5	1
	Ovarian dropsy,	4	...
	Disease of uterus	4	2
IX.	Rheumatism,	11	5
	Spine, joint, and bone disease,	16	2
X.	Ulcer,	5	2
	Fistula,	2	...
	Skin disease,	1	...
XI.	Old age,	617	115
XII.	Intemperance,	9	...
	Privation,	3	...
	Violent deaths and suicides,	129	34
	Causes not specified,	67	14
	Total deaths,	6706	955

III.—EDINBURGH METEOROLOGICAL AND MORTALITY TABLE FOR 1847.

Mouth.	Barometer.				Thermometer.				Rain n inches.	Winds general direction.	Mortality of Edinburgh.					
	Max.	Min.	Mean.	Range.	Max.	Min.	Mean.	Range.			15 years & under.	Between 15 & 60.	Above 60.	Total deaths.	Still-born.	Total.
nuary,	30·26	28·42	29·65	1·84	53°	21°	35·56	32°	0·51	E.	188	188	130	506	31	537
bruary,	30·14	28·84	29·56	1·30	51	17	36·17	34	0·79	W, N E	143	163	120	426	38	464
arch,	30·34	29 00	29·68	1·34	66	21	41·32	45	0·13	E, NE	193	169 + 1	108	471	30	501
oril,	29·90	28·76	29·45	1·14	61	22	43·35	39	1·30	W, NE	192	199 + 1	103	495	24	519
ay,	30·28	29·12	29·70	1·16	77	32	50·88	45	4·77	W, E	196	205 + 2	111	514	25	539
ne,	30 33	29·10	29·64	1·23	76	38	57·83	38	1·79	W	207	236 + 3	74	520	21	541
ly,	30·14	29·48	29·78	0·66	83	42	61·22	41	1·37	W, E	221	229 + 5	68	543	22	565
igust,	30 18	28·99	29·68	1·19	77	37	58·22	40	0·91	SW, W	200	212 + 12	80	504	24	528
ptember,	30·19	28·46	29·53	1·73	66	31	50·98	35	1·25	W	173	223 + 5	79	480	27	507
tober,	30·11	28·90	29·58	1·21	68	34	48·61	34	3·48	E, NE, W	172	207 + 7	75	521	22	543
ovember,	30·20	28·60	29·49	1·60	60	25	45·84	35	1·64	W, SW	247	359 + 10	112	728	27	755
cember,	30·05	28·14	29·38	1·91	57	21	39·06	36	3·88	E, W	307	507 + 7	180	1001	29	1030
Year.	30·34	28·14	29·62	2·20	83	17	47·42	66	21·82		2439	2957 51	1259	6706	320	7026
ean of onths.	30·17	28·81	29·62	1·36	66	28	47·42	38	1·81		203	246 + 4	105	558	27	585

Canaan Cottage, where the meteorological tables are kept by Mr Adie, Optician, is situated about a mile to the south of Edinburgh, in latitude 55° 57′, and is 246 feet above the mean level of the sea.

Harveian Society.—Notice to Students of Medicine.—The Harveian Society of Edinburgh have fixed on the following subject for their Prize Essay for the year 1849, viz.:—

"Experimental Inquiry:—On the Introduction of Medicinal and Poisonous Agents into the System by Pulmonary Absorption."

Dissertations on this subject must be transmitted to one of the Secretaries on or before the 1st day of January 1849. Each dissertation must be accompanied by a sealed letter, containing the name and address of the author, and inscribed on the back with a motto. The same motto must also be prefixed to the dissertation to which the letter belongs. None of the sealed letters are ever opened, except that bearing the same motto with the successful essay.

The prize given by the society to the successful candidate is an elegant silver medal, with a suitable inscription.

The candidate is at liberty to employ his essay afterwards, in any way he may think proper: And, accordingly, some dissertations submitted to the Harveian Society have, in consequence of publication, conferred no small degree of reputation on the authors.

No candidate who has already gained the society's prize is allowed to compete a second time.

By order of the Society,
RICHARD HUIE, M.D., 8 George Square, } *Secretaries.*
P. D. HANDYSIDE, M. D., 45 York Place, }
Edinburgh, *March* 10, 1848.

INDEX.

Abortion and sterility, Mr Whitehead on its causes and treatment 169
Addison, Dr Thomas, on the anatomy of the lungs . 192
——— *Mr William*, on the distribution of the air-passages and air-cells of the lungs . 192
Africa, report on the climate, diseases, and mortality of coast of 107
Alquié on the minute structure of the lungs . . 207
Aneurism of femoral artery, case of, cured by ligature . 481
Anomalies in muscular system, by Mr Hallett . . 1
Bladder, urinary, instance of its rupture terminating favourably 250
Blood, oil a natural constituent in 448
Blood disks, on their characters 441
Bird, Dr Golding, on urinary deposits . . 198
Böhm on the intestinal *villi* and lacteals . . 456
Bourgery, M., on the air-cells of the lungs . . . 198
Bryson, Dr Alexander, his report on the climate and diseases of the African coast . . 107
Carditis, cases illustrating . 86
Cells nucleated in the blood of the oviparous vertebrata, not in man 445
——, difference between those of tubercle and cancer . 321
Chloroform, on its use as a means of producing insensibility 243
——— unfavourable results ascribed to . . 498
Cholera at Moscow, account of 449
Climate, on that of the coasts of the Clyde . . 32
Clyde, on the climate of watering places on its coasts . 32
Cocks, Dr, his case of aneurism of femoral artery . 481
Contagion, on, as a cause of the propagation of dysentery .
Coqua leaves, on their use to produce a species of intoxication 492
Corpuscula, tubercular, on their presence and characters 317
——— of Pacini, on their distribution . 241
Cotton, Dr Richard Payne, his cases of *molluscum contagiosum* 82
Craigie, Dr, on inflammation and suppuration of the heart . 86
Crimes, how far they are to be palliated by the plea of insanity 222, 240
Davidson, Dr William, on the climate of watering stations on the Clyde . . 32
Deposits, urinary, on their pathology and treatment . 198
Disinfecting fluids, on the powers of 253
Dislocation of head of radius, case of long-continued, reduced 77
Dysentery, Dr W. Harty on, and its combinations . . 458
Epithelium, different forms of 360
Fever, epidemic, in Glasgow in 1847, Dr Orr on . . . 363
Fluids, disinfecting, on those of Sir W. Burnett and M. Ledoyen 253
Hallett, Mr C. H., on anomalies in muscular system . 1
——— on the collateral circulation in cases of obliterated veins . . 269
Harty, Dr William, on dysentery and its combinations . 458
Health of police force, Edinburgh, report on the . 292
Heart, cases illustrating its inflammation and suppuration . 86
Henle, his account of the connection between the *villi* and lacteals 454
Hewson, William, Sydenham Society edition of his works . 413
Homicide, how far it is to be palliated by the plea of insanity 222, 240
Hunter, William, his school of anatomy 413
Hysteria, tetanoid, Mr West's account of an instance of . 400
Influenza, on that of 1847 . 214
——— of 1847, official report on, from registrar-general . 378
Insanity, on, as an exculpatory plea 222, 240
Lacteals, on their origin . 451
Largs, and watering places on the Clyde, on the climate of . 32
Lebert, M., his physiologico-pathological researches on tubercles 308

Lima, on its statistics 485
Liston, Mr Robert, notice of 268
London, former state of, as regards dysentery
Lungs, on the minute anatomy of, and their air-cells 192
—— diseases of, on their mortality in 1847 214
—— state of, around tubercles 332
Mayo, Dr Thomas, his clinical facts and reflections 217
M'Cormack, Dr Henry, on oriental plague 306
Menstruation, tables illustrating the period of its cessation 253
Molluscum contagiosum, Dr R. P. Cotton's cases of 82
Montalembert, Count, on medical reform 233
Mortality, return of, for last quarter of 1847 in London 378
———— in Edinburgh 503
Mortimer, Mr William Henry, on chloroform 243
Murder, see Homicide and Insanity 222, 240
Muscular system, anomalies in 1
Negro, on the period of puberty in 69
Nucleated cells in the blood of oviparous vertebrata, not in man 445
Obliteration of veins, on the collateral circulation, in cases of 269
Oil, a constituent of healthy blood 448
Orr, Dr Robert S., on the Glasgow epidemic fever of 1847 363
Pacini, see Pacinian bodies 241
Pacinian bodies, on their anatomy and distribution 241
Peru, on its peculiar diseases
Plague, oriental, Dr H. M'Cormack, on 306
Poisoning, how far it is to be accounted for by insanity 240
Police force, report on health of 292
Pneumonia, lobular, on its peculiarities 209
Puberty, on the period of, in the negro 69
Radius, case of long-continued dislocation of, reduced 77
Reynaud, M., on the terminations of the bronchial tubes and the air-cells of the lungs 197
Rayney, Mr George, on the minute structure of the lungs 203
Reform, medical, Count Montalembert, on 233
Roberton, Mr John, his facts on the period of puberty in the negro 69
Rupture of urinary bladder, instances of terminating favourably 250
Sanitary commission, report of, on supposed cases of cholera and typhus 255
Serum, milk-like or oily, on its relations 448
Sobraon, on the wounds received in action at 51
Stark, Dr James, his case of reduction of dislocation of radius 77
—— his report on mortality of Edinburgh and Leith 263, 503
Sutlege, on the wounds received in actions on 51
Syme, Mr James, his instance of rupture of urinary bladder 250
Tait, Mr William, his report on the health of the police force 292
Tschudi, Dr Von, on the diseases of Lima 485
Tubercles, Mr George Rayney, on their origin, formation, and locality 211
———— M. Lebert's physiologico-pathological researches on 308
Tuberculosis, see tubercles and Lebert 308
Typhoid dysentery, on its existence 469
Urinary deposits, Dr Golding Bird, on their indications 198
Verrugas, a peculiar cutaneous disease in Peru
Veins obliterated, on the collateral circulation in cases of 269
Villi, connection between and the lacteal vessels 452
Vomicae, instances of the mode in which they are healed 341
West, Mr R. U., his account of an instance of tetanoid hysteria 400
Whitehead, Mr James, on the causes and treatment of sterility and abortion 169
Williamson, Dr George, his notes of cases of wounds received in actions at Sobraon and other places on Sutlege 51
Wounds, report on cases of, received in actions on the Sutlege 51

No. CLXXVI. will be published on the 1st of July.

EDINBURGH: PRINTED BY STARK AND COMPANY,
OLD ASSEMBLY CLOSE.

JUN 1969

WESBY

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